DATE LABEL

3 Dec 63
1 Jan 69
2 OCT 1974

UNIVERSITY OF KASHMIR
Acc. No 3.0.0.7.9.....
Author Read.....H.....
Title Grasmere.....Co

30079

Call No. 550.4-R22G Date

Acc. No. 30079

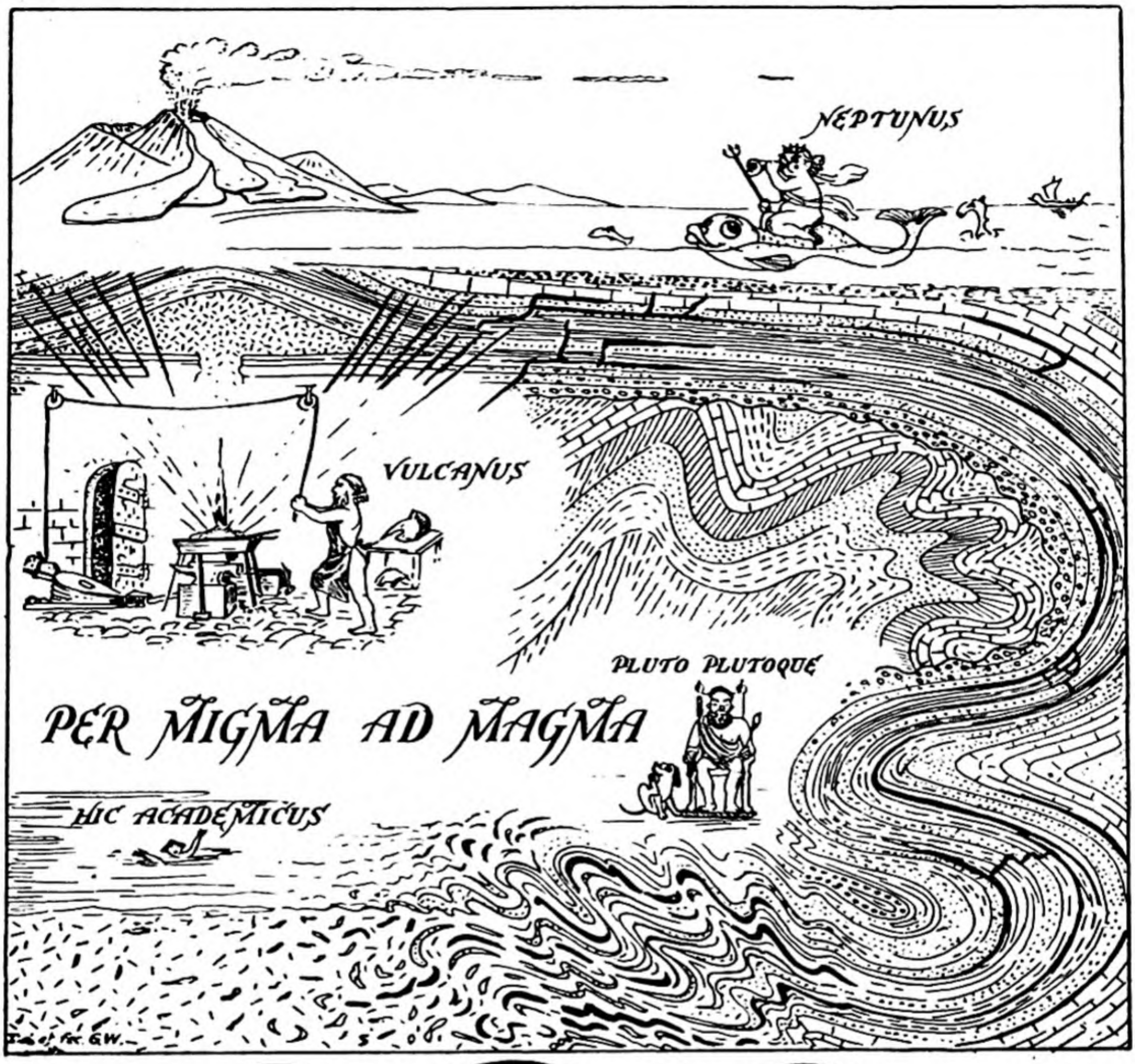
J. & K. UNIVERSITY LIBRARY

This book should be returned on or before the last date stamped above. An over-due charge of .06 P. will be levied for each day, if the book is kept beyond that day.

THE GRANITE CONTROVERSY

BY H. H. READ

Rutley's Mineralogy



The Rocks Display'd

H. H. READ

F.R.S.

THE GRANITE CONTROVERSY

Geological Addresses
Illustrating the Evolution
of a Disputant

THOMAS MURBY & CO

40 MUSEUM STREET LONDON

PROPRIETORS: GEORGE ALLEN & UNWIN LTD

FIRST PUBLISHED IN 1911

This book is copyright under the Berne Convention. Apart from any fair dealing for the purposes of private study, research, criticism or review, as permitted under the Copyright Act 1911, *no portion may be reproduced by any process without written permission. Enquiry should be made to the publisher.*

CHECKED

ALLAMA IQBAL LIBRARY
30079

ALLAMA IQBAL OPEN UNIVERSITY
No 30079
7-11-59
SRINAGAR

B50.4
L22G
R22G

BTCL
Kol

Printed in Great Britain
in 11/12 *pt. Imprint*
by J. W. Arrowsmith Ltd., Bristol 3

PREFACE

THIS volume is made up of eight addresses, concerned with the origin of the granitic and associated rocks, that were delivered between 1939 and 1954. During the early part of this period, wartime restrictions severely limited the number of offprints, and most of the addresses are now unobtainable. Reprinting in this form will provide a continuous record of the course of one side of the debate and, to put it at its lowest, the provision of a consolidated bibliography and full index will enable the reader, if he so wishes, to seek the truth for himself.

I have added a short introduction in which I have attempted to present the major points of contention, the occasion and circumstances of each address, and what I believe to be the evolution of my ideas during the discussion. Finally, I have appended a few comments on my present position.

I am indebted to the Councils of the British Association for the Advancement of Science, the Geologists' Association of London, the Geological Society of London, the Geological Society of America and the Geological Society of South Africa for permission, cordially granted, to republish the particular addresses. I wish to express my gratitude for the continuous kindnesses I have always received from these learned bodies.

H. H. READ

Imperial College
London, S.W.7
Easter 1956

CONTENTS

PREFACE *page* v

INTRODUCTION xi

I. METAMORPHISM AND IGNEOUS ACTION 1
Presidential Address to Section C, British Association for the Advancement of Science, Dundee Meeting, 1939. (*Advancement of Science* 1, 1940, 233–57.)

II. MEDITATIONS ON GRANITE: PART ONE 44
Presidential Address to the Geologists' Association, 6 March 1943. (*Proceedings Geologists' Association* 54, 1943, 64–85.)

III. MEDITATIONS ON GRANITE: PART TWO 86
Presidential Address to the Geologists' Association, 4 March 1944. (*Proceedings Geologists' Association* 55, 1944, 45–93.)

IV. GRANITES AND GRANITES 168
Opening Paper in Discussion on the Origin of Granite, Annual Meeting, Geological Society of America, Ottawa, Christmas, 1947. (Origin of Granite, *Geological Society of America, Memoir* 28, 1948, 1–19.)

V. A COMMENTARY ON PLACE IN PLUTONISM 194
Anniversary Address of the President, Geological Society of London, 17 March 1948. (*Quarterly Journal of the Geological Society of London* 104, 1948, 155–206.)

VI. A CONTEMPLATION OF TIME IN PLUTONISM 263
Anniversary Address of the President, Geological Society of London, 27 April 1949. (*Quarterly Journal of the Geological Society of London* 105, 1949, 101–56.)

VII. METAMORPHISM AND GRANITIZATION 339
Alex. L. du Toit Memorial Lectures, No. 2, delivered 25 October 1951, in Johannesburg. (*Transactions of the Geological Society of South Africa*, Annexure to 54, 1951, 1–17.)

VIII. GRANITE SERIES IN MOBILE BELTS 374

Address, delivered 16 October 1954, at the Symposium on the Crust of the Earth, Bicentennial Celebrations of Columbia University in the City of New York. (The Crust of the Earth, *Geological Society of America, Special Paper* 62, 1955, 409–30.)

BIBLIOGRAPHY 399

INDEX 415

ILLUSTRATIONS

The Rocks Display'd — *frontispiece*

1. *Suess's three zones of an orogenic belt* — *page* 213
2. *Lapadu-Hargues' curves of variation in certain crystalline schist series* — 228
3. *Lapadu-Hargues: soda migrates faster than potash* — 229
4. *Lapadu-Hargues: The iron-magnesium front* — 229
5. *Lapadu-Hargues: Distribution of different metals as a function of magmatic depth* — 231
6. *Adams's bricks* — 234
7. *The Migmatite front in the Massif Central (after Roques)* — 259
8. *The sequence chlorite, ilmenite, albite, muscovite (Cloos and Hietanen)* — 275
9. *Trends in albite (after Teall)* — 280
10. *Twisted garnets and snowball garnets* — 283
11. *Unst metamorphic history and metamorphic convergence* — 297
12. *The Thaya Window (after F. E. Suess and others)* — 302
13. *The Moldanubicum-Moravicum kata-meso-epi-zonal sequence* — 305
14. *Deformed plagioclases occurring in undeformed orthoclases (Demay)* — 314
15. *Syntectonic magmatic absorption (Demay)* — 317
16. *The relationship of migmatites to albite-gneisses in the recumbent folds of the south-east Highlands (D. L. Reynolds)* — 322
17. *The Granite Series* — 335
18. *The Granite Series of the Hercynian belt of Western Europe* — 336
19. *Sequence of Laxfordian events affecting Scourian basement, north-west Highlands of Scotland (Janet Watson)* — 380
20. *Mantled gneiss domes, Pitkaranta, Finland (Eskola)* — 382
21. *Sequence in the production of mantled gneiss domes (Eskola)* — 383
22. *Hypothetical section across north Scandinavia (Kautsky)* — 384
23. *Relations between infrastructure and superstructure in different mobile belts (Wegmann)* — 386

INTRODUCTION

GEOLOGY, as the science of earth-history, is prone to controversy. The study of history of any kind depends upon documents and records. For the history of the earth's crust, these documents are the rocks and their reading and interpretation are often difficult operations. It is true that for certain classes of rocks, made at the earth's surface, the uniformitarian method is valid and sufficient. Results produced by geological processes accessible to our observation can be compared with those preserved in rocks formed at earlier times—the present provides the key to the past. But many important rocks were clearly made deep in the crust and no uniformitarian key can unlock their secrets. Further, experimental methods can as yet scarcely be applied to them and their study becomes primarily one of field-investigation. It is this aspect of the deep-seated rocks that is the prime cause of the differences in their interpretation.

The field-geologist sees but a small percentage of the rocks exposed and this in disconnected outcrops and usually in two dimensions only. In the deep-seated rocks, he may find the dominant record to be that of one episode out of a long and complex history, and any deep-seated record may be blurred when the rock containing it is raised to the surface and so become available for our inspection. Further, the interpretation of field-work is often a strictly personal affair, depending on the observer's character, training and experience. Field-experience is often highly specialized and thus may tend to biassed general opinions; in any event, it will differ greatly among different field-geologists. From all these almost accidental circumstances, geology as earth-history has always been enlivened by debates and controversies,

often acrimonious and always vehement. Indeed, the science was born a century and a half ago in a violent discussion concerning the very rocks I deal with in these addresses. For myself, I feel no distress at these debates, provided the disputants retain what sense of humour they possess so that no friendships are broken. Indeed, I hold that innumerable and lasting benefits come from them, since inquiry and research are greatly stimulated and, at least, one's beliefs have to be re-thought. It cannot be denied that even though all that has been said in this present controversy were shown to be wrong, still the condition of this branch of geology is healthier than it was before the controversy flared up.

This provisional nature of many geological conclusions applies with particular force to those concerning the origin of one of the commonest rocks of the crust, the rock granite. However it may be formed, granite was formed in the depths, out of reach of the uniformitarian wand. The fundamental investigations of granite must accordingly be by field-work and, consequently, opinions on its origin are many and varied. As I have mentioned, the debate on the origin of granite began a century and a half ago. The Neptunists, led by Abraham Werner (1749–1817) of Freiberg in Saxony, held that granite was a chemical precipitate from a universal ocean, on whose floor it served as a foundation for the stratified rocks. James Hutton (1726–97) of Edinburgh presented the opposing Plutonist view—granite was due to the consolidation of matter made fluid by heat. Hutton observed the granite veins of Glen Tilt in Perthshire, noted that they were clearly intrusive and concluded, rightly or wrongly, that they could have originated only by the solidification of molten rock-material. Like basalt, the other common rock of the crust, granite was an igneous rock. The Wernerian theory was shattered, the problem of granite was solved.

Hutton's solution, however, rested on field-evidence, and in subsequent investigations the inherent nature of the geological method asserted itself. Depending on the type of field-evidence and on the experience and personality of the observer, granites were considered to have been formed in many different ways. If they were taken to be igneous rocks, the molten rock-substance or magma whose consolidation gave rise to them could be produced by many different processes operating in different parts

of the crust; Hutton was however universally right. But field-work showed that in some regions and there over great areas, granite was closely associated with the transformed rocks that Lyell (1797–1875) first named metamorphic, and it was proposed that granite represented a further stage in this transformation—it was of metamorphic or granitization origin. Opinions differed on the mechanism of this transformation, especially as to whether magma was involved or no. In many of the addresses in this volume, I have taken pains to present at some length the development of ideas on the origin of granite and on related topics during the nineteenth century. In these hurried days, geologists will take no harm from a quiet contemplation of the history of even this small part of their science.

In the period before the Second World War when the first of these addresses was being prepared, the majority opinion concerning granite was that it was an igneous rock produced by the crystallization of a magma. By 1930, it had been decided that this granitic magma resulted from the crystal-fractionation of a single primary magma of basaltic composition. It is true that the transformation of a pre-existing rock into granite as it stood—that is, granitization—was advocated in some measure at certain unfashionable academies, but respectable opinion held that granites were the dregs of basaltic crystallization. So far as the metamorphic rocks were concerned, it was believed that their close association in the field with granitic rocks was mostly fortuitous and had little genetic significance. Of course, as always, there were rebels against authority who pursued their lonely, but doubtless enjoyable, fancies.

Such was the general climate of opinion when I was called upon in 1939 to give the first of the eight addresses that make this volume. Since my experience had been entirely with igneous, granitic and metamorphic rocks, I sought then and have sought since no new fields. In the first address, as in those that followed, I did not attempt to present new work—as their titles sometimes stated, they were meditations, contemplations, commentaries and the like. The controversy on the origin of the granitic and metamorphic rocks was warming up and I felt that some service might be done by these periodical assessments, however partisan they might be. As was to be expected, my opinions on these matters

underwent a measure of evolution. In what follows in this introduction, I propose to trace briefly this evolution and, finally, to inquire where it has now brought me.

In 1939, I prepared the first address, *Metamorphism and Igneous Action*, as President of Section C of the British Association at the Dundee meeting in September of that year. The address was, in fact, never given; it was down for delivery on 4 September, but war was declared on 3 September. The title, at least, was a suitable one for Lyell's native county, and the address began quietly as a study of the nature and the causes of regional metamorphism and its connexion with igneous action. But as it proceeded, the address got rather out of hand and it ended with doubts about whether the action that was involved was igneous at all. Regional metamorphism, it was suggested, was related to migmatization and granitization and was expressed by zones of metamorphism about a granitization core. The presence of a granitic magma was assumed or, at least, not closely questioned.

The next two addresses, the somewhat notorious *Meditations on Granite*, were given to the Geologists' Association. They were prepared under wartime conditions in the basement of the Royal School of Mines in London. Their preparation, in the congenial company of firewatchers from cleaners to professors—in that order of decreasing efficiency, I am afraid—occupied the long dull periods between more exciting occurrences. In these two addresses, I attempted to define some of the key-words such as granite, igneous, magma, granitization, migma, that appear so frequently in the granite debate. I decided that igneous rocks were those formed by the consolidation of magma, fluid rock-material, and discussed the possible origins of granitic magma. My conclusion was that if granitic magma was necessary, it was primary in its own right and was not derived by crystallization-differentiation from basaltic magma; granite and basalt were different things. After an examination of the field-evidence, I held that granitization was a reasonable process and I suggested that mobilization and intrusion led to granites of various kinds but of one connected origin. There were granites and granites; I was approaching the notion of a Granite Series that I developed

in later addresses. On the historical side, I completed the review of the evolution of ideas that I had begun in the Dundee address—especially presenting and annotating the work of the French and Fennoskandian schools. In dealing with this work, I made much of '*dents de cheval*' as evidences of granitization; as we see later, I put these into a truer perspective after a visit to South Africa. My friend, Dr. Gilbert Wilson, kindly provided me with the cartoon, *The Rocks Display'd*, reproduced as the frontispiece of this volume. This cartoon has appeared in many books, and I have encountered it on the walls of geological departments in all parts of the world. I suspect that it will be remembered when my addresses are completely forgotten. It gives with perfect clarity my classification of rocks into Neptunic, Volcanic and Plutonic—a return to Lyell. The plutonic rocks—the granitic, migmatitic and metamorphic rocks—are associated with orogeny and contrast with the volcanic rocks, non-orogenic, dominantly basaltic and truly igneous.

Immediately after the Second World War, when interchange of wartime publications was renewed, it became evident that a first-class controversy on granites and granitization was on. The Geological Society of America fittingly staged a day's discussion on the origin of granite at the Annual Meeting at Ottawa at Christmas, 1947. Through the generous hospitality of the Society, I was enabled to open the debate. My contribution, *Granites and Granites*, reprinted as the fourth address in this collection, was followed by four others by proponents of diverse views and then the whole freely discussed by a score or more members. The day was a memorable one, probably unique in the history of geology. The proceedings are recorded, often verbatim, in Memoir 28 of the Geological Society of America and provide, especially in some parts of the discussion, most entertaining reading.

Granites and Granites was a kind of trial balance of my views at that date, developed from the three earlier addresses and modified by the consideration of newer work become accessible after the War. I found the granite-basalt separation increasingly attractive, and I still regarded '*dents de cheval*' with favour. New aspects of the granite debate were the proposals of fronts, especially the basic front, and of solid diffusion as a mechanism of transformation. There had become apparent the beginnings of a cleavage amongst the transformists or granitizers between the *Wets* who

utilized pore-fluids and other liquids and the *Drys* who advocated solid diffusion. But perhaps the most important personal development was my tentative groping after a Granite Series, as expressed in my title, 'Granites and Granites'. Many of the disputants at this Ottawa meeting were greatly concerned about how much granite was of transformation origin and how much magmatic. This obsession provoked me, in the discussion, to present the Granite Series. This Series had elements of time and of space; it showed the varying relationships of style of granite, nature of metamorphism and temporal and spatial position within the orogeny. Level of exposure controlled the amounts of the different kinds of granitic rocks.

In 1948 and 1949, I was President of the Geological Society of London and, taking advantage of the presidential prerogative of speaking at large and in the general, I proceeded to study my own geological evolution in two addresses on plutonic geology, with place the dominant theme in the first, time in the second. I broadened the granite discussion into one on plutonism, this meaning the production of the whole connected group of granitic, migmatitic and metamorphic rocks. In *Place in Plutonism*, number five in this collection, I dealt with the 'stratigraphical' elements of the plutonic rocks—schistosity, foliation, zones and facies. The cleavage between the Wets and the Drys among the transformists had by now become more marked; I decided to be cautious about the dry proposals. The basic front continued in favour. The following year, in *Time in Plutonism*, I applied time to the geometry of the previous year's inquiry and dealt with plutonic and metamorphic history, the time-relations between granitization, migmatization, metamorphism, intrusion and orogeny. By now, the Granite Series had become a well-established tenet of mine, as I considered that it presented a unified sequence of plutonic events that could be followed through time and place in an orogenic belt. The essential element of time had been applied to the geological map of plutonic rocks.

In the autumn of 1951, I was invited to give the Du Toit Memorial Lecture in Johannesburg before the Geological Society of South Africa and kindred societies. I chose as my subject *Metamorphism and Granitization*, an echo of the first address in this volume. As a prelude to my lecture, the Geological Society

of South Africa, the Union Geological Survey and the South African universities kindly made it possible for me to spend a couple of months inspecting a wide variety of plutonic regions in all parts of the Union. Under these stimuli, my geological evolution continued apace. By now it had become a fundamental part of my position that the validity of any granitization could be judged only by the regional setting of the granite body at the actual level at which we now see it; this is, of course, essentially a restatement of the Granite Series. This position was confirmed, I held, by the beautiful phenomena shown in Zululand, Namaqualand and the Cape Province. The vast migmatite-metamorphite terrains of the first two regions were the scenes of large-scale granitization and incipient mobilization; in the Cape plutons, high and late in the Granite Series, there was little granitization, the granite bodies getting into place by softening-up their contacts. The '*dents de cheval*' at Sea Point, Cape Town, were now taken as local phenomena, not to be applied by themselves to the interpretation of the pluton as a whole. Whether these felspars could be formed in two different environments or only in one no longer seemed to me to be fundamental in the granite controversy. Further, the importance of basic fronts in granitization appeared to be small, and I preferred to consider the basic behinds or subtraction-rocks as of greater significance. The examination of differential granitization in a varied rock-series led to the notion of resisters, and of barriers holding up the advance of metamorphic fronts; as a result, I restated my scepticism concerning any standard set of metamorphic zones—the watchword of the metamorphic geologist should be *sui generis*. In the marvellous exposures of migmatites in Natal and Namaqualand, mobilization could be established as a real process and, after seeing granites and granites in abundance and variety throughout the Union, my affection for the Granite Series showed no signs of lessening. Finally, I considered that the origin of the emanations responsible for granitization was in the sima; but granites and basalts were still quite different things, not to be connected within the accessible crust by any crystallization-differentiation process. Possibly lightheartedly, I suggested that the granitization of the earliest sediments by emanations from the sima might be the explanation of the discontinuous sialic rafts of the continents.

Columbia University, New York, attained its bicentenary in 1954 and among the many celebrations of that happy event was a symposium on the Crust of the Earth organized by the Department of Geology. I was kindly invited to take part, my allotted subject being the *Granite Series in Mobile Belts*, reprinted as the last address in this volume. In this, I examined the mobility in the basements resulting from their granitization and inquired into the subsequent history of granitic rocks during the orogenic episodes. The relations between time, place and setting of all the granitic rocks in an orogen, that is a Granite Series, were again considered to be both logical and elegant. As the fundamental cause of both granitization and orogeny, down-buckling was advanced; some of the melted sima appeared as the greenstones of the geosynclinal piles, the crystallization of the rest provided the emanations to granitize the geosynclinal sediments and as the orogeny proceeded, the chemical mobility was followed by mechanical mobility of the granitic rocks. Granites and basalts were different things, but their ultimate source was one.

It may be of interest to make an assessment as to how my geological evolution has developed during the course of the granite debate. In the first place, I consider that the separation of the granites from the basalts as recording two fundamentally different geological events is sound and is likely to lead to fruitful results. Secondly, it has become apparent, to me at least, that the geological setting of the plutonic rocks is a matter of profound significance in their interpretation. The origin of any granite body can only be reasonably discussed if its associates, its size, its structural and temporal relations at its present level are also taken into account. I have come to attach little importance to basic fronts and '*dents de cheval*' as criteria of the origin of any given granite body. Thirdly, I have a better appreciation of the dimension of time in plutonic geology, especially that of the granitic and metamorphic rocks. The relationship between plutonic phenomena and orogeny has taken on a rational pattern; the Granite Series may not answer all questions but, at least, it seems to me to make the questions simpler.

As a result of these disputations, I believe that no granite

problem now exists. Each granite is a unit to be discussed by itself, to be related to its setting and to be interpreted on its intrinsic evidence. There are, as I have always held, granites and granites, but they are all logically related to one another. This *via media* position of mine has been derided by extremists of both the right and the left; one such has recently stigmatized it as being an elegant way of hurting nobody's feelings—I am sure, however, that any other position would hurt my own feelings and I see no reason to do that. All of us in this debate started off as violent partisans of one cause or another, but few have remained so. Most of the extremists on both sides have come to occupy a middle position and little now divides us. I requote, as I have done so often, the passage from Hutton which is as true now as when he made it in 1795:

'While man has to learn, mankind must have different opinions. It is the prerogative of man to form opinions; these indeed are often, commonly I may say, erroneous; but they are commonly corrected and it is thus that truth in general is made to appear.'

I

METAMORPHISM AND IGNEOUS ACTION

1939

Und dass mich zuweilen bedünken will, gar manche der viel-gepriesenen Wunder des Metamorphismus dürften recht passenden Stoff zu einem 16. *Buche der Ovidischen Metamorphosen liefern*

C. F. Naumann, 1858

ON many counts it is fitting that the subject of the metamorphism of rocks should be dealt with at a meeting in Dundee. In the first place, on 14 November 1797, there was born at Kinnordy, near Kirriemuir, less than a score of miles north of this city, that self-confessed 'loudest and most indefatigable squaller of all the brats of Angus' who gave us the term itself—I refer, of course, to the illustrious Charles Lyell. Again, beyond Kirriemuir and the low-lands of Strathmore, which are underlain by the Old Red Sand-stone, most Scottish of rock-formations, begin the Highlands, composed for the greater part of metamorphic rocks. Time was when marauders from these fastnesses raided the fatter lands of the south, and a body, worthy perhaps of a more dignified title, once penetrated as far as Derby. It is a matter of observation that their modern and, on the whole, more polite descendants still take the English road and, moreover, have reached still farther south. But when we consider the geological traffic, the movement is and always has been in the reverse direction. For a century, English geologists have been attracted by the unlimited scientific riches of the Highlands. They have penetrated as yet only as single spies; they will come I hope as battalions—for there is loot for all. Who knows but that the true value of this invasion from the south far outweighs the more material spoils which are sent northwards by the Scots?

Among these English invaders, I wish to single out George Barrow (1853–1932) who, in the Highlands of Angus Kincardine and Aberdeen, north of this city, made discoveries so far in advance of his time that they have only recently been rediscovered. Barrow made mistakes too; let those who would belittle his work on this account ponder Edward Greenly's 'Orders of Validity in Geological Theory' (Greenly, 1926)—but he contributed many major ideas to metamorphic geology, ideas concerned with metamorphic zones, with filter-press action in igneous injection, with the relation between metamorphism and igneous intrusion, and so forth. Bearing in mind the main theme of these present remarks, I desire to pay this tribute to Barrow's work and memory.

To Lyell, who introduced the term in 1833, 'metamorphic' meant transformed. In that he coined a special word, it seems clear that he did not intend all rock-transformations to be included in metamorphism. Some modern extensions of metamorphism to cover weathering, cementation, and the like appear to destroy whatever justification Lyell's special term had. From the beginning Lyell considered that metamorphism was closely connected with some kind of igneous activity. The metamorphic rocks 'had been modified by plutonic agency under pressure in the depths of the earth'. Again, 'they must lie at the bottom of each series of superimposed strata, because the influence of the volcanic heat proceeds from below upwards'. In the *Principles* it is stated that, in metamorphism, 'the transmutation has been effected by the influence of subterranean heat acting under great pressure, and aided by thermal water or steam and other gases permeating the porous rocks, and giving rise to various chemical decompositions and new combinations'. In the *Elements* of 1838, the importance of magmatic gases is clearly expressed. Such gases are considered to have permeated the earth's crust in a belt 'thousands of fathoms in thickness', and it is suggested that 'their heating and modifying influence may be spread throughout the whole of this solid mass'. Lyell's views, written a century ago, bear a strangely modern look.

In the development of ideas on metamorphism since Lyell's time, we see emphasis laid upon the dominance in rock-transformation of one physical factor or the other, as in the expressions dynamic metamorphism and thermal metamorphism, or upon

spatial considerations, as in regional metamorphism or local metamorphism. The content of metamorphism becomes enlarged to include weathering and the like, or contracted to exclude simple crushing. Recrystallization is regarded as an essential in metamorphism by one master, and as not requisite by another. According to one school, the chemical composition of rocks undergoing metamorphism is not changed; according to another it can change to almost any extent. The definitions of the term itself, and of the phases of metamorphism, are the subjects of dispute. This lack of uniformity, which still exists, may give pain to some, but for my part I regard it as a sign of vigorous growth—metamorphic geology without controversy would be moribund. Moreover, I appreciate fully and often a remark of Jacques de Lapparent's, 'le bon sens d'aujourd'hui, qu'on nomme parfois vérité, n'est pas nécessairement le bon sens de demain'.

This diversity of opinion is due to certain fundamental and human causes. The development of an observational science like geology depends upon the personal experience of individual workers. I suggest that, with certain reservations that need not be considered at this moment, the best geologist is he who has seen most rocks. In the rather intricate field of metamorphic geology, specialization comes early and continues. A metamorphic geologist who has investigated a Scottish injection-complex, for example, spends his vacations visiting injection-complexes in Finland, the Pyrenees, and elsewhere: his view of the whole field of metamorphism is coloured by his knowledge of injection-complexes; if the reader persists to the end of this address he may recognize an example of this.

This limited experience, in even the most competent geologists, has left its mark on the flux of ideas on metamorphism. We find there the personal experience of the masters, restricted though it must be, giving a bias to their concepts of the whole field of rock-transformation. This led to the rise of schools of thought which were, curiously enough, markedly nationalist in character. A brief examination of these national schools will serve to show the development of metamorphic geology, so far as my special topic is concerned, prior to the Great War.

In 1877, Rosenbusch of Heidelberg published his classic account of the thermal or contact metamorphism around the Barr-Andlau

granite in the Vosges. He found there no evidence for transfer of material from the intrusive granite into the surrounding country-rocks, and proceeded to apply his conclusions to all granite contacts: permeation by magmatic juices was impossible. The great authority of Rosenbusch led to the adoption of this doctrine not only in Germany, but also in other countries; we find it still lingering in our own. Meanwhile, Lossen (1875), Lehmann (1884), and others in Germany had dealt with rocks in which movement was held to be responsible for their metamorphism, and Lossen had proposed the term dislocation metamorphism for what Rosenbusch later called dynamic metamorphism. For Rosenbusch and the orthodox German school, therefore, there were two kinds of metamorphism—first, contact metamorphism of Barr-Andlau type, and second, dynamic metamorphism in which pressure was the operating factor, this pressure being essentially orogenic in character. Even in Rosenbusch's own time, some of his compatriots, such as Lehmann himself, resisted his dictatorship, and during the first decade of the present century many examples of transfer at granite contacts, injection and assimilation, had been described by German workers, e.g. Chelius, Erdmannsdörffer, Gäbert, Gürich, Klemm, Lepsius, Philipp, Sauer, Schwenkel, Steinmann, Weber, and others.

In France there had developed meantime a powerful school with tenets completely opposed to those of Rosenbusch. As early as 1844, Virlet d'Aoust had used the expression *roches d'imbibition*, an expression that has played a great part in French ideas on metamorphism ever since. From their studies, during the 80's and 90's, of granite contacts in the Pyrenees, Brittany, and elsewhere in France, Michel-Lévy, Lacroix, Barrois, and others concluded that the country-rocks adjacent to the advancing magma had been changed in chemical composition by the influence of mineralizing agents, volatiles and solutions, so that finally they approached granite in character. The original granitic magma (not to be confounded with the granite rock) combined with the country-rocks either by imbibition (permeation) or by lit-par-lit injection. Passages from country-rock through mica-schists of various types to granite-gneiss, gneissose granites, and finally granite, were described. The original granitic magma advanced by conversion of its country-rock into granitic material and the

incorporation of this into the main body of moving magma. Granitization or felspathization was established as a petrogenetic process.

Many of the products of these injection and granitization processes were foliated or gneissose in structure. According to the French, therefore, gneisses were not the result of a dynamic metamorphism in which the source of the heat was to be sought in the mechanical work produced during folding, as Lehmann and other German authorities believed, but such rocks derived the heat necessary for their formation from the uprise and injection of magmatic material. As Michel-Lévy remarked, contact metamorphism at considerable depths becomes confluent with regional metamorphism.

The bitterest opponent of dynamic metamorphism among the French, however, was Termier (1904, 1912). In 1904 he declared, 'le dynamométamorphisme n'a pas maintenant d'adversaire plus acharné que moi', and proposed that the term should be abolished. Dynamic action deformed, but it did not transform, and mylonitic rocks were its only product. Termier's views with regard to regional metamorphism and granitization differed in many respects from those of his countrymen. He agreed that no one could reasonably doubt that in the formation of the true crystalline schists there had been an afflux of elements from the depths which had chased the old elements before them, and that many gneisses, especially those containing xenoliths (segregations according to Termier) had attained some slight degree of fluidity which permitted an orientation of their components. He agreed also that the production of the crystalline schists was concerned with depth of burial in the geosyncline, but that depth alone was not sufficient. The causative factor was the arrival from below, in *colonnes filtrantes*, of juvenile liquids bringing with them various gases, silicates and borates of the alkalies; the temperature of the geosynclinal sediments increased rapidly, eutectic mixtures went into solution first, the old elements in excess of the eutectic proportions fled before the *colonne filtrante* and displaced other elements in their turn; true magmas were formed here and there—of all dimensions, increasing with depth; in the upper parts, the geosynclinal sediments were recrystallized without change of chemical composition, and passed downwards into gneisses and, upwards

and laterally, into less metamorphosed rocks; the still liquid 'magmatic' portion was intruded at higher levels in cross-cutting form. Termier employed his famous metaphor: the production of regional metamorphism can be likened to the unequal spreading of a spot of oil in a pile of cloths, the spreading being governed by the different permeabilities of the layers of the pile. Regional metamorphism, therefore, was not caused by igneous intrusion; it was not a more extensive form of contact metamorphism. When regional metamorphism and igneous intrusions occurred together, they were two effects of the same cause, the rise of the *colonnes filtrantes*. The production of igneous rocks was only an episode in regional metamorphism. In Termier's beliefs we see the germs of many doctrines of today.

Turning now to the contributions from Austria and Switzerland, we must acknowledge the pre-eminent work of Becke of Vienna, especially in the petrography of the metamorphic rocks. In dealing with the crystalline schists of the Waldviertel (1912) he wrestled with one of the great problems of metamorphic geology—a problem that we shall encounter again and again in this address—and that is, the coincidence of schistosity with primary structures such as bedding, and the horizontal attitude of such metamorphic rocks over considerable areas. He found it difficult to explain the widespread horizontal schistosity by tangential pressures, but the parallel structures seemed scarcely understandable without invoking directed pressure of some kind. He suggested the operation of something akin to contact metamorphism—not related to any visible igneous body, but rather due to the hot interior of the earth; only local injection metamorphism was observed by him in the Waldviertel. Here we have a return to ideas as old as Hutton. Thirty years before, Becke (1882) had considered that two types of dynamic metamorphism were recognizable, one at shallow depths, which gave rise to propylitic rock-types (the epi-schists of present-day nomenclature) and another at much greater depths which yielded products like those of contact metamorphism. In 1913, in one of the most outstanding contributions to metamorphic geology of any age or country, he developed these ideas on depth-zones—ideas which now play so fundamental a part in the study of the metamorphic rocks. In 1878 Heim of Zürich had distinguished between an upper zone of the crust in which rupture

took place, and a lower zone of plastic flow; twenty years later, Van Hise of U.S.A. examined the physical-chemical factors in the two zones, the upper of fracture, the lower of flow, and showed that in a general way reactions took place in the upper zone with liberation of heat and increase of volume, and in the lower zone with absorption of heat and decrease of volume. Becke, in his remarkable paper of 1913, showed how the mineral associations and textures of metamorphic rocks depended upon the pressure-temperature conditions prevailing at the time of their formation. He utilized the Volume Law, first proposed by Lepsius (1893), to demonstrate that increase of pressure led to the formation of minerals of higher specific gravity; pressure increased with depth. Temperature, too, increased with depth, and the most important cause of this was the approach to the internal heat of the earth. Becke gave two zones, an upper, where the formation of hydroxyl minerals was possible, and a lower, where hydroxyl minerals could not be formed. Becke's zones have been expanded, elaborated and defined, chiefly by Grubenmann and Niggli of Zürich. In the upper, or epizone, temperature was low, hydrostatic pressure small, directed pressure either very strong or lacking, and the style of the metamorphism is dominantly mechanical with the formation of hydrous silicates; in the mesozone, the temperature was higher, hydrostatic pressure higher, directed pressure weaker and the metamorphic style is characterized by chemical reactions; in the deep, or katazone, temperature was very high, hydrostatic pressure very high, directed pressure weak or absent and the metamorphic style is characterized by the presence of many minerals akin to those of igneous rocks and by a but-poorly-developed schistose texture. Suggestions of the depth-control of metamorphism are as old as Lyell, and references to shallow and deep zones of metamorphism are abundant through-out the history of metamorphic geology. In my opinion, the association of depth with metamorphic zones of this type, as in the expressions depth-zone and Tiefenstufe, may occasionally lead to unfortunate results. I return to this topic in later pages, but I may remark here that *Tiefenstufe* at least does not occur in the index to Grubenmann-Niggli's *Die Gesteinsmetamorphose* of 1924.

I return to Switzerland in my historico-nationalist amble through Europe, to mention certain ideas first of Weinschenk and

then of Koenigsberger. Weinschenk (1901, 1902), in dealing with the granitic gneisses of the Alps, was concerned with a very old question indeed, namely, whether such gneisses are completely primary igneous rocks. The intrusive masses must have exercised a metamorphic action upon the adjacent rocks, and the sedimentary cover retained its original structures on this metamorphism—a circumstance readily explained by contact metamorphism, but rather difficult to harmonize with dynamic action. The difference between ordinary contact metamorphism and the Alpine metamorphism of Weinschenk is the evidence for the activity of great pressures during the latter and the tendency of the rocks produced thereby to occupy less volume; recrystallization after folding is often demonstrable. To explain these observations, Weinschenk suggested that orogenic pressures were operative during the intrusion and solidification of the granitic magma. Under such oriented pressures, the magma kernel was compressed, the early-formed minerals were oriented, and minerals of smallest molecular volume were formed—all typical of a process styled piezo-crystallization by Weinschenk. Further, as the magma continues to solidify under pressure, the residual volatiles are expelled at a high temperature into the already folded sedimentary cover where they contribute to its metamorphism—a process of piezo-contact metamorphism.

Weinschenk suggested that regional metamorphism is largely an affair of piezo-contact metamorphism. These views did not find favour with Becke, who gave an alternative explanation—the metamorphism followed immediately on the consolidation of the magma and therefore the transformation was effected in the presence of abundant juvenile water and other mineralizers; in such an environment, thorough metamorphism is easily achieved, whilst mechanical phenomena will be lacking. Further, Berg (1910), for example, considered that the original crystallization from a fluid under pressure, which can only be hydrostatic, cannot be proved to be essentially different in structure from that under normal circumstances.

Koenigsberger (1912*a*) proposed an interesting classification of the crystalline schists. The formation of crystalline schists containing such minerals as andalusite, cordierite, sillimanite, garnet, staurolite, biotite, orthoclase, &c., with sieve-structure, results

from a lengthy intrusion of igneous material at a high temperature. Such rocks (let us say those of Becke's lower zone, or of Grubenmann-Niggli's meso-kata-zones) are grouped with migmatites as contact-metamorphic schists. Koenigsberger has a second group, labelled dynamo-metamorphic schists, which includes first, mylonites and second, true regional-metamorphic schists. This last-named subdivision is an interesting one; it includes the rocks of Becke's upper zone or of Grubenmann-Niggli's epizone. Koenigsberger suggests that they should be called tele-intrusion-schists. They are formed, as it were, in the outermost parts of aureoles in connexion with the movement of igneous masses. It seems to me, therefore, that with the exception of mylonitic and similar rocks, the rocks of all metamorphic zones would be considered by Koenigsberger to be connected with igneous activity.

We may now proceed northwards into Scandinavia, where contributions of the first magnitude were being made to the study of metamorphism. In Finland, Sederholm (e.g. 1907, 1913, 1923, 1926) in a lengthy series of masterly memoirs based on detailed field-observation, had dealt with the complexities of the Finnish Pre-Cambrian, and had come to occupy a position very similar to that of Michel-Lévy, Lacroix and Termier in the French School. He opposed Rosenbusch and the 'micropetrological school of Heidelberg'; could not agree wholeheartedly with Weinschenk's views on piezo-crystallization; regarded Koenigsberger with suspicion and, as we shall see immediately, looked with complete distrust on certain Swedish interpretations of the Archean. Metamorphic geology was flourishing in the North.

Though Sederholm was one of the first to regard grade of metamorphism as a function of depth, he rejected the Volume Law and conclusions based upon it. Garnet, for example, could not be employed as a geological barometer. The importance of pressure in metamorphism had been grossly exaggerated: 'the original features often remain so unchanged that not even faults of a millimeter's length may be detected'. Cataclasis is a destructive process: it does not create new minerals and rocks; Sederholm considered the use of the term dynamo-metamorphism to be far too wide, and recommended that it should be employed only when the facts of the case warranted. Agreeing with the French School, he recognized that solvents are essential in metamorphic

changes and that they are exudations from igneous magmas. The metamorphism of the Finnish Archean was therefore regional contact metamorphic.

Sederholm, during his long field-experience, dealt with a series of granite contacts of astounding complexity. He preferred to take granite magma as available, without considering how, where or when it might have been formed. By processes of penetration and injection, in which the magmatic juices or ichors played a large part, the granitic magma mixed with the country-rocks to give a great variety of mixed rocks,—the migmatites. Sometimes the magma advanced by Termier's *tâche d'huile* mechanism; certain permeable layers of the bedded rocks became metasomatically changed to rocks of typical aplitic or granitic composition. The process of forming granitic masses by emanations from an abyssal magma was called anatexis,—a re-fusion or re-solution; often a rock undergoing anatexis was endowed with a new capacity for intrusion—such reborn 'magmatic' rocks were styled palingenetic. The Finnish migmatites, like their French analogues, are often striped and foliated gneisses and inherit these textures from the original schistose rocks which have been replaced during the granitization. Anatexis and the formation of migmatites occurred at various times in the deep Archean zone of Finland, where Sederholm suggests that denudation may have penetrated 100,000 feet, or even more.

These anatectic processes were considered by Sederholm to surpass ordinary metamorphism in potency and thus to come within what Holmquist of Sweden had called ultra-metamorphism. Anatexis implies that part of the material of the younger rock has been derived from an older rock. The question as to the importance of this part developed into a discussion between Sederholm and a Swedish school led by Holmquist, Hogbom and others. Sederholm considered the veins of the veined gneisses (his arterites) as of igneous origin, to be connected in all cases with some known granite plainly in evidence. The vanguard of pegmatitic veins advances before the great army of the granitic magma, and in some cases joins with revolutionary forces of the country invaded. In the opinion of the Swedish school of ultra-metamorphism, however, the veins were mainly exudates from the surrounding rocks—an application of the classic lateral secretion notion; they

are due to the action of revolutionaries, operating in their own country, on their own initiative, and without material help from outside. Holmquist (1907, 1910, 1920, 1921) regarded the production of veined gneisses (his venites) as a further stage of regional metamorphism, and related to the inner deformation of the rocks: they were ultra-metamorphic. True arterites, i.e. those related to igneous injections, were quite subordinate in amount, and were absent from most igneous contacts. Holmquist agreed that in regional metamorphism the rocks were flooded with solutions, and that in areas where folding and magmatic injection were contemporaneous, conditions for 'pegmatitic' vein formation are ideal—high pressure, high temperature, and abundance of mineralizers. The difference in principle between this conception and Sederholm's anatectic re-fusion at great depths seems to me to be slight.

In his consideration of venites, however, Holmquist was not concerned only with gneisses penetrated by veins of pegmatitic aplitic or quartzo-felspathic composition, but dealt also with veins in all kinds of rocks. He showed that many veins are related in composition to the rocks in which they occur—quartz veins in quartz-rich rocks, calcite-bearing veins in calcareous rocks, quartzo-felspathic veins in the Swedish quartzo-felspathic leptites; clearly lateral secretion had been at work. He demonstrated thereby the processes now styled metamorphic diffusion and differentiation —processes studied especially by Stillwell (1918), Eskola (1932*b*), Read (1933, 1934*b*), McCallien (1934), and others. Further, in discussing the palingenetic formation of pegmatites in high-grade gneisses, Holmquist referred to the 'real fusion of the most fusible rock masses', suggested that quartz and felspar are the first to liquify, and recalled the earlier proposals of the Americans, Van Hise, Lane and Daly. This principle of selective re-fusion, again, is one which in the hands of Eskola, Wegmann, Backlund and others, has come to play an important part in petrogenesis and metamorphic geology; I refer to it on a later page.

Before dealing with the British contributions to the development of the special aspects of metamorphic geology which concern us here, I make necessarily brief reference to contributions by North American geologists. During the period which I am considering now—up to about the Great War—American investigations

of special interest to us fell into two main classes: the one essentially American and concerned with transfer at igneous contacts, the other dealing with injection metamorphism and granitization after the manner of the Scandinavians and French.

The association of certain ore deposits with contact metamorphism had been described in the Banat district of Hungary by von Cotta as early as 1865, and later von Groddeck proposed a group—those of Christiania type—formed through processes of contact metamorphism. At the turn of the century, many large contact deposits were described and discussed from the American Cordillera and the mechanism of these pyrometasomatic deposits was placed upon a firm basis. The magnificent work of Lindgren and a score of other mining geologists in the great mining camps of Arizona, Utah, Montana and adjacent states showed that such deposits had been formed at high temperatures by emanations from igneous intrusions, usually of granodioritic or monzonitic character, which passed in great volume into the adjacent limestones. At the contacts, skarns, associations of iron-rich silicates, calc-silicates and ore-minerals, were formed; in some cases these skarn minerals were later than the normal contact-metamorphic minerals, in others no time difference was recorded. Transfer at igneous contacts was unequivocally demonstrated. The work of the American mining geologists was of first class importance, also, because it threw light on the processes of metasomatism or replacement, and demonstrated the control of pressure and temperature in the formation of ore deposits due to emanations from igneous magmas.

Turning now to the second group of American contributions, we may first recall that J. D. Dana in 1886 considered that gneisses had been formed by metamorphic action in which a state of refusion had been reached; admittedly, Dana thought too that many other rocks, granite, norite, diorite and so forth, had arisen by the same process. The most important work of this time, however, was that of Lawson in the Lake of the Woods and Rainy Lake areas of Canada. Lawson described annular belts of mica-schists around granite-gneiss cores; the foliation of the cores was due to crystallization whilst the magma was moving slowly under direct pressure, and to the same dynamic action he ascribed the production of the schistosity in the sedimentary envelope—though

he cites innumerable cases where schistosity is parallel to original bedding. The granite magma was due to fusion in the deeper parts of the geosyncline. In 1910, F. D. Adams and Barlow published their important monograph on the Haliburton and Bancroft areas of Ontario, where conditions were in part very similar to those found by Lawson. They saw no reason to reject Lawson's conclusions concerning the formation of granitic magma by re-fusion and the production of foliation in the granitic gneisses by movements during intrusion of this magma; they attributed the regularly banded gneisses to streaking-out of amphibolite inclusions during such movements. They considered that transfusion (their word) of certain constituents of the granite magma into the limestone had given rise to amphibolites and pyroxene-gneisses of various kinds and instituted a comparison with Lindgren's pyrometasomatism. Though they correlated their results with certain observations by Lacroix in the Pyrenees, they were unable to admit that sediments could be changed *in situ* into granite. Finally, we may quote Adams's statement at the 1910 Geological Congress at Stockholm: 'we have in the great volume of igneous magma rising from the deeper portions of the earth's crust one of the primary agencies, if not the chief agency, of metamorphism, which metamorphism may manifest itself, although with decreasing intensity, for great distances from the actual intrusion itself'. At this same congress, Coleman declared that no case was known in Canada where metamorphism was entirely due to deep burial; igneous rocks were always present in or beneath the sheet of sediments. These views may be contrasted with those of Van Hise (1898), who admitted that the schists surrounding batholiths were identical with those considered by him to be produced by regional dynamic action; he held that the requisite conditions—movement under sufficient pressure, moderate temperature and presence of water—were available in the two cases in quite different ways.

Before leaving this topic of the origin of gneissose banding, I desire to make reference to the stimulating paper of Fenner (1914) on the mode of formation of certain gneisses in New Jersey. Fenner described in detail a limited area of an injection-complex. He showed that the dark minerals of the granitic part had been derived from the country-rocks, that the banded gneisses were

composite rocks formed by lit-par-lit injection and by absorption of magmatic material by the country-rock, and that directed pressure had not been operative. He dealt with the question of reconciling the existence of large thin tabular inclusions, standing erect and unsupported in the granitic rock—and thus denoting a viscous magma—with the undoubted permeation which demanded extreme fluidity, and concluded that gradual substitution had taken place rather than the violent intrusion usually pictured. The invasion of the magma was preceded by the advance of a wave of metamorphism into the country-rock, whose composition was changed, both by the deposition of magmatic minerals and by the removal in solution of certain of the old constituents, till it approached that of the magma itself; blocks of country-rock, so changed, were easily assimilated by the magma. Fenner emphasized the importance of gas-fluxing on the country-rock, a topic to which he returned in 1933, as mentioned at a later stage in this address; a rock with pores too small to admit the main magma could admit magmatic gases.

I wish now to deal with the British contributions on the special topics of this address. We can consider in the first place Judd's concept of statical or static metamorphism, though it is true that we shall wander very far afield in this consideration. Judd (1889) recognized a tendency in his time to over-emphasize the importance and validity of dynamic metamorphism. He proposed his term statical metamorphism to cover those changes resulting from pressures which do not effect movements in the rock-masses, and are therefore related to the weight of a superincumbent load; such changes went on at great depths and under enormous pressures. It is important to realize that Judd considered that such static metamorphism had as its most potent agency the penetration of the whole mass of the rocks by various liquid and gaseous solvents.

Judd's concept was discussed and elaborated by various geologists in Europe and America. A most important contribution was that of Milch (1894) of Breslau, in which he distinguished between dislocation metamorphism and load metamorphism. In the former, pressures tangential to the earth's surface were concerned, mechanical phenomena were dominant and the cause was patently tectonic. On the other hand, load metamorphism resulted from

the vertical pressure of the overlying beds and no mechanical effects were evident. Contrasted with the local and irregular development of dislocation metamorphism, the effects of load metamorphism were widespread. Milch drew attention once more to the old problem—the metamorphism of undisturbed beds and the coincidence of schistosity and bedding—and solved it by load metamorphism.

In America, Van Hise (1898) employed static metamorphism to cover molecular dynamic action where interchange between molecules had taken place, rather than the deformation characteristic of mass dynamic action or dynamic metamorphism. According to Van Hise, the distinctive features of static metamorphism were the growth of large individuals, irregular grain-size, and the preservation of original textures. The question of the validity of static or load metamorphism has been discussed at length by Daly (1917) of Harvard, in his examination of the phases of metamorphism. Dawson (1901) had suggested that the widespread horizontality of the crystalline schists of Eastern Canada and certain features of the Shuswap rocks of British Columbia had 'been produced rather beneath the weight of superincumbent strata than by pressure of a tangential character accompanied by folding'. Daly (1915, 1917), in his independent investigation of the Shuswap terrain, came to the same conclusions concerning the efficacy of load metamorphism. He quoted a score of examples of areas of crystalline schists from all over the world where no evidence of crustal deformation was to be observed, where the inclination was low, foliation and bedding were coincident, and where the grade of metamorphism was not related to the degree of deformation. All these phenomena Daly felt to be 'truly inexplicable by pure dynamic metamorphism', but readily explained by load metamorphism. In particular, the parallelism between schistosity and bedding, no matter what the attitude of the metamorphic rocks may be, is difficult to explain by the tangential force of intense orogenic movements. I return to the topic of load metamorphism on a later page.

Before considering the work of George Barrow in Scotland, I may first recall the studies of certain other British investigators in injection-complexes and their views on granitization and related topics. I have no time here to do more than mention the ideas of

Clifton Ward (1875, 1876) and A. H. Green (1882) on the metamorphic origin of granite. Green suggested that 'bedded' granite had arisen *in situ* by metamorphic processes, whilst 'amorphous' granites, in which bedding had been lost, marked a more advanced stage of metamorphism; intrusive granites were attributed to a still greater degree of energy in the transformation process. Very similar notions concerning the origin of the Lake District granites had been propounded by Ward. As we shall see, these ideas are still active though in a modern dress.

In 1896, Horne and Greenly gave an account of a portion of the great injection-complex of Sutherland in the Northern Highlands —an injection-complex with which I have been intimately concerned in recent years. They showed that a series of phenomena were coincident, namely, increases in quantity of granite in the complex, in its intimacy of relation with the country-rocks, in the grain-size of the crystalline schists and in the abundance and perfection of sillimanite. They concluded that the causes which brought about the introduction of the granite also resulted in the high-grade type of metamorphism. They believed, however, that though the granite injections were closely associated with the metamorphic processes they yet most likely found the country-rocks already crystalline schists. In discussing the origin of the foliation of the granites, they considered that the biotite folia, the foliation-makers, were relics from the country-rocks, the quartzo-felspathic elements of which had been incorporated in the granite magma.

Horne and Greenly quoted (as I did with great approval in 1925) from a report (1893–94) of Hugh Miller, junior, who dealt also with the Sutherland complex. This quotation, though lengthy, is well worth repeating:

'The structures in the granites and granitic gneisses . . . are now found to be to a large extent imitation-structures, due to a simulation of the forms and structural features of the country-rock . . . by granites that have by some means crept into their place. The process by which this replacement has been effected seems to have been, to a considerable extent, a development of crystalline matter among the granulitic materials of the pre-existing schists and quartzite. In the earlier stages of the metamorphism the granitic substance has entered or by some means suffused the structure of the stone, appearing first (in the more

porous parts of the country-rock, as it would seem) as a fine mottling of granitic particles. In further stages of the metamorphism the granitic matter, keeping for the most part to the folia of the pre-existing rock, has increased in knots and knotty strings, has entered planes, slide-planes, and the lines of contortion in contorted schists (without any apparent signs of crush), and so, filling the stone, thickens into bands and sills at the expense of the original rock, until the latter, almost altogether replaced, is represented only by inclusion-planes, and ultimately by inclusion-structure. In the granites thus produced there is simulated, so far as crystalline matter can do so, the fine layering of some parts of the [country-rocks], and the coarse banding of others: there is also preserved *in cast* the rumplings and striation of their surfaces, and to some extent also apparently their lines of cleavage . . . the inclusion-structure (as well as the inclusion-planes) everywhere retains the same dip and strike as that of the country-rock. . . . Parts of these granites are in fact, to some extent not yet defined, pseudomorphs or granite casts, preserving within portions of their mass, as *replacement structures*, the remains of the structures of the pre-existing rock.'

B. N. Peach, my revered predecessor in this Chair, used to put it more briefly: the foliation in these rocks, he would say, is simply the grin of the Cheshire Cat.

In a later paper dealing with the permeation of schists by granitic material, Greenly (1903) supported the view that the extension of the magma proceeded by quiet diffusion rather than by forcible injection; only by such a supposition could thin plane rafts of country-rock in the granite be explained. Here was an early expression of Fenner's arguments to which reference has already been made. Teall, that wise man, in the discussion on Horne and Greenly's paper, observed that though every inch of the rocks of Sutherland was alive with movement, yet the crystallization must have taken place after the movement had ceased. Grenville Cole (1902) in his investigation of the composite gneisses of Western Ireland reached a position similar to that of the Scottish workers. The banded gneisses there were formed by the union, both mechanical and chemical, of igneous and stratified material, and the original structure of the bedded rocks was inherited by the gneisses. Later Cole referred to the production of banded gneisses as due to contact metamorphism on a regional scale.

I now take up in such detail as time allows the work of George Barrow in the Highland region north of this city. In a series of papers, reports and memoirs, appearing between 1892 and 1913, Barrow developed certain major ideas on the relation between igneous intrusions and regional metamorphism. He separated out two main epochs of granitic intrusion in Angus and Kincardine, that of the Older Granites, a series of intrusions intimately associated with the production of metamorphism of one kind or another, and that of the Newer Granites, intruded entirely later than any metamorphism and responsible for only a local and low-grade alteration of the crystalline schists already formed. Certain portions of the Older Granite magma, such as the Ben Vuroch granite, consolidated before the general metamorphism; the hornfelses due to this early stage were for the most part destroyed by the crushing movements of the general metamorphism, but their relics were to be found in all areas flooded by this early magma. Later intrusions belonging to the Older Granite series raised the whole area to a very high temperature and were thus responsible for the high-grade metamorphism of the rocks in which they occur. These later portions of the Older Granite magma appeared in an enormous number of small intrusions, and in places they permeated the country-rocks and formed with them injection-complexes of various types. During the consolidation of many of these portions, pressure was operative, and from the partly crystallized magma were squeezed out the liquid residuals. By this mechanism there arose a north-western belt of oligoclase-biotite-gneiss, representing the material already crystallized when the pressure came into action, and a south-eastern belt of pegmatitic rocks, rich in microcline and muscovite, representing the strained-off potash-rich fraction. Differentiation by filter-press action had been at work.

At an early stage in his investigations, Barrow observed that in the areas flooded by the main Older Granite magma, the country-rocks showed an extremely high grade of metamorphism characterized by sillimanite-gneisses. Just as the Older Granite itself changed, as already noted, from the north-west to the south-east, so did the grade of metamorphism: the high-grade sillimanite zone of the north-western belt was followed south-eastwards by lower grades, first that with kyanite and then that with staurolite.

Barrow (1893) considered it 'reasonable to attribute both the minerals and the crystallization to the thermometamorphism of the intrusion'. He finally established seven zones of increasing metamorphism, marked by the incoming of an index mineral in rocks of pelitic composition. Going in from the Highland Border towards the central areas of Older Granite, he mapped out the following: (1) zone of clastic mica, (2) zone of digested clastic mica (chlorite zone of present-day nomenclature), (3) biotite zone, (4) garnet zone (almandine zone of today), (5) staurolite zone, (6) kyanite zone, (7) sillimanite zone. This progressive series began with slates affected by pure dynamic metamorphism and finished with coarse-grained sillimanite-gneisses associated with the Older Granitic intrusions. The whole series of zones was to be regarded as in the nature of gigantic contact-aureoles around intrusions of the Older Granite.

By this remarkable series of investigations, Barrow made three outstanding contributions to metamorphic geology, namely, those concerning filter-press action, zones of progressive regional metamorphism, and the association of high-grade metamorphism with granitic intrusions. Some of his conclusions, especially those dealing with his early hornfelsing and with the origin and metamorphic history of many of his granitic gneisses, are, in my opinion, definitely wrong, but his work has been the basis on which much modern progress has been established, and I desire, as I have done before, to pay a sincere tribute to his pioneer genius. I love, too, a bonnie fechter*; and he was one of the bonniest. Like all of us, he suffered from his enthusiasms; the Older Granites were all powerful, whilst the gigantic intrusions of Newer Granite were permitted to perform nothing but a low-grade metamorphism. This aspect of Barrow's tenets was seized upon by his colleagues, and I trust I shall be forgiven, both by their author and by the manes of Barrow, if I quote two lampoons on this question, which I have abstracted from the (unofficial) archives of the Geological Survey of Scotland.

The first is a verse of a lengthy poem in which the eminent members of the Scottish Geological Survey of Barrow's time are adequately dealt with. It runs:

* Bonnie fechter: *Scots*, good fighter.

'Mr. Barrow proclaimed with great volume of sound
That the Old Magma flourishes all the world round.
This intrusion the schists all around it does roast,
And when it is absent it alters them most.'

The second extract is headed *Brief for Defendant in New* v. *Old Magma*, and states:

'Take an imaginary line as straight as an arrow and as broad as long from Timbuctoo to Chicago—the hornfelsing is developed all along that line.

'If the Older Granite, or any agent acting therefor, appears anywhere along that line, it proves that the hornfelsing is caused by the Older Granite.

'If the hornfelsing is absent from any or all points of the aforesaid line, it is negative evidence and proves nothing.

'If the hornfelsing occurs anywhere else than on the aforesaid line, it is not real hornfelsing.

'If the Newer Granite appears on or about the line, it proves that it has nothing to do with the hornfelsing.'

Whilst we smile at these japes now, let us hope that posterity will deal with the ideas of each of us as kindly as present-day metamorphic geologists are dealing with Barrow's.

I have now come to the end of my travels through the space and time of the development of metamorphic geology up to the nineteen-twenties. I have endeavoured to place before you the pertinent contributions from the nations. I have, for obvious reasons, been compelled to make a selection, and in these queer times I am constrained to apologize to any individual or nation that feels aggrieved at its nature. We may now pass on to a discussion of the major ideas so briefly stated, and of their modern derivatives.

The student of metamorphism learns very early that there is no kind of uniformity in the classification and definition of rock-transformations. He finds one dichotomy based upon space, as in regional and local metamorphism, another on mechanics as in static and dynamic or static and kinetic metamorphism, a third

on geological considerations, as in Grundgebirge and Deckgebirge metamorphism, and so forth. Furthermore, when he considers any particular type of metamorphism, he realizes that whilst it may be possessed of an honest-looking title, yet this honesty is liable to become somewhat blown-upon when the numerous definitions concerned are examined.

This is specially evident in connexion with the term regional metamorphism, where several branches of different dichotomies become intertwined. The only common denominator in definitions of regional metamorphism—and one at least I feel we are entitled to expect—is that such metamorphism affects rocks over extensive areas. This having been admitted by all, then alleged genetic considerations come into play.

Some geologists, Daubrée and Termier for instance, require that regionally metamorphosed rocks should arise by the action of hot emanations on deeply buried rocks; others, including Rosenbusch, Holmquist and the British School as represented by Teall and Flett for example, use the term as equivalent to dynamic metamorphism; Harker considers that the essential of regional metamorphism is a conjunction of high temperature and intense shearing stress; and still others, such as Geikie, Kemp and Clarke, maintain that the definition should state clearly that the transformation was not connected with igneous activity. Daly (1917), who has wrestled valiantly with these matters, approves of this last definition—regional metamorphism is that 'not caused by eruptive bodies', and its opposite is local metamorphism 'caused by eruptive bodies'. For my purpose here, I propose to take the expression 'regional metamorphism' as meaning only and exactly what it says, namely, a transformation that has affected large portions of the earth's crust.

I have noted in the preceding paragraph the equivalence, according to Rosenbusch and others, of regional and dynamic metamorphism. Whilst I do not feel so bitter about dynamic metamorphism as Termier did, still I hold that its efficacy as a process of rock-transformation has been exaggerated. As we have seen, the forerunner of the term dynamic metamorphism was Lossen's dislocation metamorphism, a simple expression denoting a transformation genetically connected with dislocations of the crust. It is true that Lossen demanded that heated water should

be active as well, but if we agree, as some (notably Van Hise and Riecke) do, that any metamorphism is impossible without the activity of solutions, then Lossen's demand was not unreasonable. Lossen's old term would cover a large class of phenomena, such as the dominantly mechanical breaking-down of rocks, cataclasis, mylonitization and the like, all connected with demonstrable dislocations. It must be remembered, however, that some authorities, Daly being among them, consider that recrystallization is essential in all metamorphism, and exclude purely mechanical transformations. I do not sympathize with this exclusion and propose to continue to use dislocation metamorphism for that class of phenomena I have mentioned.

Rosenbusch, having replaced Lossen's excellent term by his dynamic metamorphism, proceeded to make this the equivalent of regional metamorphism and to regard orogenic pressure as its cause. Backed by the authority of Rosenbusch, dynamic metamorphism became fashionable, the dynamic aspect overshadowed all others and mountain-building movements could do all things requisite and necessary. It must be remembered, however, that Judd, Sederholm and others urged caution and, of course, many of the French were frankly sceptical; nevertheless, the notion of a metamorphism on a regional scale 'induced in rocks because of their deformation' remains still a leading principle in many schools of metamorphic geology. Sometimes we have the notion in its purest form. For example, at Flin Flon, Canada, where a progressive metamorphism has achieved garnet, the cause is ascribed by Ambrose (1936) to heat developed by shearing.

On the other hand, the emergence of ideas on a static or load metamorphism of regional extent, in which orogenic pressure is not a causal condition, indicates that all is not well with the supposed equivalence of dynamic and regional metamorphisms. Before discussing static metamorphism itself, I propose to deal with some of the difficulties raised by this equivalence. I take as my definition of dynamic metamorphism that given by Daly (1917)—'metamorphism which is induced in rocks because of their deformation, the crustal movement being of the orogenic type'; it is a division of regional metamorphism since it is 'not caused by eruptive bodies'.

In an earlier part of this address, where reference is made to the work of Becke, Weinschenk, Sederholm, Milch, Daly and others, I mentioned certain facts commonly observed in regionally metamorphosed rocks but difficult to reconcile with the operation of pressures tangential to the earth's surface. One of the chief of these observations is the coincidence of schistosity and bedding. As metamorphic rocks showing this coincidence are often horizontal over great areas, the notion of static or load metamorphism has been put forward. But, as I have already mentioned, Daly (1917) has cited a score of regions where schistosity and bedding agree—in folded and horizontal rocks alike. These are the phenomena that Daly felt to be 'truly inexplicable by pure dynamic metamorphism', an opinion with which I am in complete accord. It is true that some observers have questioned whether the banded appearance of many crystalline schists is to be interpreted as original bedding. For instance, it has been suggested that this banding may be due to flow of softened heterogeneous masses, or to the operation of some kind of differential diffusion whereby different components become lodged preferentially in certain bands. Again, Barth (Balk and Barth, 1936), for example, in his account of the metamorphism of the Palaeozoic rocks of Dutchess County, New York, looks with suspicion on the interpretation of layers of different composition in metamorphic rocks as representing original stratification, and prefers to emphasize the importance of shear-planes and subsequent crystallizations along them in the production of schistosity and foliation. While these, and other, explanations of banding may be valid for certain areas, still I believe that schistosity and stratification are coincident in a vast number of cases. My belief is strengthened by the considerations dealt with in the next paragraph.

The preservation of original textures in rocks that have been completely recrystallized is a matter of common observation. There are innumerable examples of the preservation in regionally metamorphosed rocks of minute sedimentation characters, such as graded bedding, current bedding, the interlamination of undisturbed, exceedingly thin beds, and so forth—characters that should have been obliterated by the action of orogenic pressures. As an instance of how excellent this preservation may be, I may recall the successful use of current bedding and graded bedding

by Bailey, McCallien, Read and others in unravelling the stratigraphical sequence in the Grampian Highlands. An additional and telling example is the recognition of varved bedding in the metamorphic rocks of Finland and Canada. It seems to me that such details of original textures cannot possibly be preserved if the rocks in which they occur were metamorphosed 'because of their deformation'. I admit, of course, the operation of dynamic action in the production of various metamorphic rocks of low grade, but in these cases the original primary textures are completely obliterated. Van Hise (1898) emphasized this aspect of dynamic metamorphism and held that complete destruction of original textures will result from comparatively little motion. I may cite from my own experience the entire absence of any sedimentary textures, apart from those afforded by a conglomerate-schist bed, in the Muness Phyllites of Unst in the Shetlands. This obliteration in low-grade metamorphism and preservation in high-grade metamorphism of original textures shake my faith in the unity of the progressive series from the chlorite zone or epizone to zones of higher grade—but this topic I reserve for a later page. Whether the topic arises at all depends, of course, upon the validity of the argument that the banding of metamorphic rocks represents in most cases an original bedding, and I consider the validity unquestioned.

It may be objected that beds of different competencies will react differently to orogenic stresses, and that, for example, whilst original textures may be preserved in quartzose members they may be destroyed in pelitic rocks. Admittedly some accommodation may take place in the clay layers; if this is effected during the metamorphic epoch, rotated porphyroblasts may be the result. The Abbildungskristallisation of Sander which we may translate as mimetic crystallization and describe as a crystallization in which the original textures, bedding, &c., are in control of the orientation, is so common a phenomenon that it cannot be disregarded.

Whilst stress is admittedly the dominant factor in the production of low-grade metamorphic rocks, there is agreement that its effect is small in the higher grades, where the products of regional and thermal metamorphisms converge, high temperatures being in control. There is no correspondence (*pace* Ambrose and others)

between the degree of deformation and the metamorphic grade, unless it be one of the greater the deformation the lower the grade. In this connexion, I may cite the failure of experimental work, like that of Larsen and Bridgman (1938), to produce minerals characteristic of rocks supposedly formed by dynamic metamorphism. Stress, by itself, is not enough.

No one can dispute the observed fact that regionally metamorphosed rocks are often more or less violently folded. The inference so often made that the metamorphism and the folding are coeval is not, however, always justified. It seems to me clear, on the basis of the immediately preceding paragraphs, that most fold-structures in regionally metamorphosed rocks antedate the crystallization, this being post-tectonic in the terminology of Sander.

I have dealt here with the contention that deformation causes regional metamorphism. It is necessary to remember that the converse, that regional metamorphism causes deformation, has been held to be true. I can only draw attention to Perrin's (1935) recent discussion of this aspect, and make mention of Mellard Reade's expansion theory of mountain-building and of Bailey Willis's theory of metamorphic orogeny. Reade supposed that heating and metamorphism of the sediments in the deeper parts of the geosynclines gave rise to an expansion which expressed itself in folding and granitization. This is essentially what was later suggested by Joly. On the other hand, Willis saw in the pressures exerted by the oriented growth of crystals during metamorphism a sufficient cause for orogenic folding.

Even from the foregoing inadequate discussion, it will be realized, I trust, that there are many features of regional metamorphism that are incompatible with dynamic action. The recognition of this has led to the invocation of static or load metamorphism, and I propose now to examine the validity of this notion. First, however, I must quote Daly's definition: 'static metamorphism is that phase of regional metamorphism which is not induced by orogenic deformation'; load metamorphism is the subdivision of static metamorphism which takes place at high temperatures. Remember that, according to Daly, regional metamorphism is 'not caused by eruptive bodies'.

One of the especial difficulties that has to be faced by advocates

of a metamorphism due to the vertical pressure of the overlying beds is the existence of completely non-metamorphic rocks which have nevertheless been covered by an immense thickness of superincumbent strata. From the innumerable examples available, I select a few. Chamberlin estimates that the Palaeozoic rocks of Pennsylvania which have been involved in the Appalachian folding have been under a cover of from $4\frac{1}{2}$ to 6 miles in thickness; in spite of this great depth of burial and of the intensity of deformation, they show none of the characters of regional metamorphism. Daly has noted the almost complete absence of recrystallization in the Lower Cretaceous rocks of British Columbia, though they have been beneath a cover of 8,000 m. of sediment. Larsen reports 50,000 ft. of non-metamorphosed sediments in California; Arnold Heim notes 9 km. in a Chinese geosyncline; Schuchert estimates 25 km. in the Rocky Mountain geosyncline, and O. T. Jones nearly 40,000 ft. in the Welsh Lower Palaeozoic geosyncline. It is clear that vertical pressure due to load is not a prime cause of regional metamorphism.

This difficulty confronting static metamorphism has been tackled by Daly, who meets it by relaxing the rigidity of the doctrine of uniformitarianism. He admits that, compared with its proposed potency in Pre-Cambrian times, load metamorphism must have been of relatively little importance in later geological eras. To account for this, he assumes that the earth's thermal gradient was steeper during the formation of the Pre-Cambrian so that regional metamorphism under a moderate cover was possible. He considers that this speculation concerning a hotter surface to the earth is 'no more dangerous than the fashionable explanation of all, or nearly all, regional metamorphism by orogenic movements'. I agree that though uniformitarianism suits the events of the 500 million years of geological history as recorded in the Cambrian and later fossiliferous rocks, it may quite likely not be so valid for the 2,000 million years of Pre-Cambrian time. I am not competent, however, to discuss Daly's speculation, and I have only to state that I feel that the proposal does not strengthen the case for regional static metamorphism.

The idea of a metamorphism controlled by load, as such, fails to meet many other observations. I have space here only to cite the occurrence, in the eastern United States, of highly metamorphosed

stratigraphically younger beds resting naturally on lowly metamorphosed stratigraphically older beds; of high-grade sillimanite-zone rocks overlying lower-grade rocks as in Sutherland and Norway; of the passage, in Banffshire, from slates to metamorphosed rocks containing andalusite, garnet, cordierite, sillimanite and staurolite within a half-mile of coast-section; of a similar narrow restriction in the Barrovian zones as in Dutchess County, New York. In my opinion, the effect of load completely fails to account for these observations. Load, by itself, is not enough.

The literature of regional metamorphism abounds in references to the great depths within the crust at which such metamorphism takes place. These suggestions of great depths, however, carry no kind of conviction to my mind. On the contrary, I consider that high-grade regionally metamorphosed rocks must have been formed in many areas under relatively little cover. Daly was of this opinion too, and he was influenced thereby in his idea that thermal conditions were different in Pre-Cambrian time from what they were in Palaeozoic time. He pointed out that from Clarke's data it is possible to form a rough estimate of the total amount of rock eroded in geological time, and that only a small portion of this amount can be assigned to Pre-Cambrian time. Of this small portion, part is represented in the non-metamorphic Pre-Cambrian sediments which lie unconformably on the metamorphosed basement. The several complexes of the basement were highly metamorphic before the denudations corresponding to the unconformities which separate them. From a consideration of these points, and of the great volume of Pre-Cambrian rocks, Daly concludes that the average cover on the complexes at the time of their metamorphism was much less than 5,000 m. in thickness. Barrell (1921) came to a similar conclusion by an argument based upon the amount of salt in the sea and of the erosion of the igneous rocks to give this. He decided that Pre-Cambrian erosion had removed a cover of less than a mile in thickness.

Though these arguments may not appear altogether sound to some, still I suspect that the notion of the great depths of regional metamorphism flourishes because of the supposed necessity of carrying rocks *down* to be metamorphosed. I suggest, and I develop this suggestion later, that as an alternative we should

consider, as Joly also would do, the possibility of bringing the metamorphosing agents *up*.

Following on these remarks on load metamorphism, I may conveniently here call attention to the recent work of F. E. Suess of Vienna on the relation between regional metamorphism and tectonics. Suess considers that in a typical orogen there are three separate zones—an outermost, the non-loaded, zone of folded non-metamorphic rocks, then an intermediate zone, the loaded zone, where the rocks are violently folded and metamorphosed by the activity of a creative block (the *traîneau écraseur* of Termier). This creative block, which constitutes the third, or innermost, zone, is moved to form the load upon the loaded zone. Examples may make this classification clearer: in the Caledonids of this country, the non-loaded zone is represented by the folded Lower Palaeozoic rocks of Wales, the Lake District and the Southern Uplands, the loaded zone by the Dalradian and the creative block by the Moines; in the Variscan orogeny of Central Europe the creative block of the Moldanubian has given rise to the regional metamorphism of the loaded zone, the Moravian; in the Alps the load is the Austrids, the loaded zone the Pennids. Two fundamental types of regional metamorphism stand in dependent relation to the tectonics of the creative block, or the load, on the one hand, and of the loaded zone on the other.

The first zone, the load, is characterized by a type or facies of regional metamorphism, styled periplutonic by Suess, which is closely associated with batholithic intrusions and essentially thermal and non-stress in type; magmatic transfer and solution have led to the formation of katazone rocks with a post-tectonic mimetic crystallization; this metamorphism is not controlled by depth. Periplutonic metamorphism has affected very extensive areas lying outside the true orogenic or folded belts of the crust, and its domain passes downwards into the region of migmatization and anatexis. Along stripes of deformation, the katazone rocks may be degraded to meso- and epi-zone types, whilst adjacent to the major dislocations a thorough reconstruction may take place through a considerable thickness of rock, giving a type of metamorphism which Suess has recently called hypokinematic. Examples of periplutonic metamorphism are provided by the coast

region of southern Finland, the Moldanubian and, though not quite typically, by the Moines of Scotland.

The second, or loaded, zone shows a different type of regional metamorphism, named enorogenic, which is controlled essentially by deformation due to the passage over it of the creative block. Because of the load of this block, isotherms rise in the loaded block below, but the metamorphism of this is mainly due to the setting-up of tangential pressures. Rocks characteristic of enorogenic regional metamorphism are meso- and epi-zonal in type, and high-grade rocks which become involved in this metamorphism are degraded to these lower types. The metamorphism is patchy, metamorphic 'unconformities' are common, and the crystallization is often paratectonic, that is, crystallization and folding are synchronous. During enorogenic metamorphism there is an immense activity of alkaline solutions which, acting along the multitude of shear-planes, lead to the production of much white mica—a sort of widespread shimmer-aggregate formation. Enorogenic regional metamorphism is confined to the orogenic belts, and is exemplified in the Scandinavian Hochgebirge, the Moravian block and in the Dalradian of Scotland.

From Suess's work it appears that two different supplies of heat are to be considered: first, that produced by the damming-back, as it were, of the earth's internal heat by the thickened cover of the load and effective in enorogenic regional metamorphism, and second, that directly transferred by magma and active in periplutonic regional metamorphism.

Suess bases his conclusions on a series of comparative studies of metamorphism and orogeny in numerous areas, many of which he has visited, in Europe and America. I have no space here to enter into even a general discussion of his views. From my personal knowledge it would be no difficult matter to criticize in detail their application to the Moine and Dalradian rocks of Scotland, but such a criticism by itself would be grossly unfair. A proper test of Suess's interpretation as applied to the Scottish Highlands could be based only on an investigation of the natural history—as I have called it elsewhere—of the metamorphic rocks of that area. British geologists have not considered seriously the possibility of polymetamorphism in the Highland rocks, but, faced with Suess's suggestions, they must do so in the future. It is with

some such hope that I have devoted these few paragraphs to an account of Suess's work.

Even the inadequate summary here presented serves to indicate why Suess could not regard the epi-, meso- and kata-metamorphisms of Grubenmann and Niggli as forming a continuous series of rock-transformations gradually increasing in grade, nor could he consider the corresponding zones as separable according to depth since they are fundamentally dependent upon the dynamics of different tectonic controls. He concluded, therefore, that there is no regular series of metamorphic zones; the zones are not particularly connected with one another and can each occur quite independently of the others. In an earlier page of this address, I have expressed some misgivings of my own on the unity of the progressive series of zones. I propose now to discuss certain aspects of the zonal notion, especially in connection with a depth-control.

I have already noted the development of ideas on depth-zones and, in connexion with static and load metamorphism, I have directed attention to difficulties encountered in regarding grade of metamorphism as directly controlled by depth. Admittedly, metamorphism does often increase in grade with depth, but, as I have said in another place, I personally find no difficulty in envisaging a metamorphism which increases laterally or vertically; I consider depth, as such, not to be a factor in metamorphism and that if we are to retain a zonal notion, then the idea of depth-control must be completely disregarded.

The view that depth and grade are genetically related has played what I fear to be an unfortunate part in the interpretation of many metamorphic terrains. I can here only instance the Grampian Highlands, where, according to Tilley (1925) and Miss Elles (1930), the metamorphic zones have been inverted over considerable areas since their formation. This interpretation seems to me to be based fundamentally on the conclusion that, no matter what may now be the attitude of the zones, they were originally formed with the highest grade at the greatest depth and with an orderly structural succession from high-grade to low-grade zones from the deeper parts upwards. Even if this fundamental postulate be admitted, and I for one refuse to admit it, still I find it difficult to understand how large-scale inversions of metamorphic zones

have taken place without the rocks concerned acquiring a new metamorphism due to, and essentially contemporaneous with, the inversion. One might suggest that the portion of the crust showing such phenomena had been inverted as it were on a hinge, but the requisite hinge would be of so gigantic a size that both the crust affected and the suggestion must collapse. Where, as in the North-West Highlands, rocks of various kinds have been involved in relatively small-scale inversions, they show a metamorphism produced during the inversion. I may recall here, too, that T. Vogt (1927) in Norway and I (Read, 1931) in Sutherland have found high-grade rocks resting on lower-grade rocks and have felt content to consider this the position in which they were originally formed.

The relation between the large-scale tectonics and the metamorphic zones in the Highlands is a question which, in the present state of our knowledge, bristles with difficulties. As we have just seen, the Cambridge School consider that the metamorphism is pre-tectonic and that the metamorphic zones can be recumbently folded. On the other hand, Bailey and others regard the metamorphism as partly contemporaneous with the folding. For my part, I suggest, for reasons which will appear later, that the metamorphism may be post-tectonic. Both in the Highlands and elsewhere it seems to me that the isograde lines are independent of both stratigraphical and tectonic arrangements, and I prefer to relate regional metamorphism not to load nor to deformation nor to tectonic or any other depth.

I have at various places in this address pointed out the destruction of sedimentary structures in low-grade metamorphism and their apparent perfect preservation in high-grade metamorphism. If these are valid observations, then it seems to me that the progressive series from slates to higher-grade rocks must break down or, at least, that high-grade rocks were not necessarily at one stage of their career in the condition represented by the lowest-grade rocks of the zonal series. Low-grade rocks may once have been high-grade, but the reverse is not necessarily true. I cannot admit that such rocks as the high-grade delicately striped hornblende-granulites of Sutherland, which most likely are of sedimentogeneous origin, or the metamorphosed varved rocks of Finland, for example, ever passed through a stage in which the

dynamic factor was overwhelmingly dominant. Becke's early classification of regionally metamorphosed rocks into the propylitic type and those resembling products of contact metamorphism or Milch's separation of his dislocation metamorphism from load metamorphism may express real differences, and for these classifications, when shorn of their depth or load aspects, I have some sympathy.

Apart from the question of the time continuity of the progressive series represented by, say, the Barrovian zones, there is the chemical continuity to be considered. How far do the zones of Barrow represent an isochemical series of pelitic rocks? The degree to which such processes as metamorphic differentiation and diffusion have operated obviously affects the compositions of members of the series at any stage. Quartz and quartzo-felspathic segregations withdraw material from the unit of rock undergoing transformation, and other components indigenous to the rock may migrate from place to place within it. The staurolite-zone of Barrow seems to demand a special chemical composition for its formation, and this composition might be provided either by the original sedimentary composition or by enrichment in iron and alumina through impoverishment in magnesia by metamorphic diffusion. Barth's (1936) interpretation of the extreme composition of the Bamble formation in South Norway by the expression of low-melting components is concerned with a notion similar to that contained in the latter half of the previous sentence. Such possible departures from the isochemical series arise through processes inherent in the rock. There is, however, another aspect of this topic which, though usually ignored in this country, must in my opinion be carefully considered. The possibility of the introduction of material of so-called magmatic origin is one that certainly cannot be dismissed in the higher grades, and one that might apply even to the lower grades of regional metamorphism. The remaining portion of this address is largely concerned with this possibility. It will be convenient for the development of my argument, however, if we transfer ourselves to the domain of ultra-metamorphism, and I propose first of all to discuss the reality of granitization, for, if granitization is real, certain consequences seem to follow.

In a paper which has appeared during the writing of this address,

granitization is defined by Malcolm MacGregor and Gilbert Wilson (1939) as 'the process by which solid rocks are converted to rocks of granitic character'. It includes all such operations as palingenesis, syntexis, transfusion, permeation, metasomatism, migmatization, injection, assimilation, contamination, and the like. I have already made mention, in previous pages, of earlier views on this topic in connexion with the French School, Termier, Sederholm, Fenner, Hugh Miller, and others. More recent developments have been ably summarized by MacGregor and Wilson. They make reference to the investigations of Goldschmidt in Stavanger, Norway; of Read in Sutherland and Aberdeenshire, Scotland; of Quirke and Collins in Eastern Canada; of G. H. Anderson in the Inyo Mountains of California and Nevada; of Barth in Dutchess County, New York, and of others; they refer, too, to their own work in Scotland and Yugoslavia. I mention these authors and localities to show how completely international the idea of granitization has become. MacGregor and Wilson discuss the chemical data supplied by these investigations, and the trends of the progressive changes in composition as granitization proceeds. They conclude that exchanges take place between the country-rocks and their pore-fluids—whether these are entirely indigenous or reinforced by accessions from magmatic sources—and that these exchanges are selective so that there is a convergence, both chemical and mineralogical, of rocks originally different. They consider two processes to be concerned in granitization; first, a metasomatism under the influence of 'permeating highly energized fluids—emanations—ahead of advancing magma', and second, a mechanical penetration by magma. I consider that no reasonable objection can be raised to these conclusions, notwithstanding the uncompromising attitude of Rosenbusch and others. It seems to me that this Franco-German granitization war, at least, has been won by the French.

The origin of granitic magma is admittedly a problem closely related to granitization, but nevertheless one which does not directly affect the validity of this process. To what extent migma can become magma is a matter for individual judgement. We have truly a varied choice of schemes for the production of granitic magma. We can believe that it exists as a primary earth-magma, or that it can be produced by differentiation from a primary

basaltic magma, or by the fusion of the granitic layer of the crust, or by paligenetic melting by basal fusion of sediments involved in regional subsidence, or by solution of the low-melting components of crustal rocks and the expulsion of the solution so formed, or by rheomorphism, that is, the conversion of sedimentary rocks into mobile masses by the action of emanations. We can derive the energizing fluids involved in granitization from a magmatic source and permit them to produce granitized rocks and even new granitic magma; or we can accept the emanations as available without questioning their ultimate source and see in them the primary agent in the production of magmatic rocks. Whatever mechanism we regard as reasonable, I consider it demonstrated in dozens of localities that 'solid rocks are converted to rocks of granitic character'.

The question of room in migmatization is one that has caused difficulties to many observers. For example, Milch drew attention to series of sediments which remained the same thickness over considerable areas even though they were said to have been injected by much igneous material; he preferred to regard the felspathic material seen in such series as arising from the rocks themselves and not contributed from outside. This room-question was answered by Hugh Miller fifty years ago, and has been answered many times since. Replacement is the essential process in the formation of the migmatites. As Hugh Miller said, 'parts of these granites are, in fact, pseudomorphs or granitic casts, preserving within parts of their mass, as replacement structures, the remains of the structures of the pre-existing rocks'. It is unfortunate that the term injection-complex, used in this country, emphasizes injection, since permeation, imbibition and metasomatism are more widespread phenomena. As I have pointed out elsewhere, the formation of banded gneisses and lit-par-lit complexes is best explained by a process of replacement to different degrees along layers of different permeabilities. The many examples of the tracing of the regional country-rock structure through granitic masses, the existence of thin plane screens of country-rock in them, the great extension of minute *lits* of quartzo-felspathic material in migmatites, and the detailed heterogeneity of many granites are all readily explicable by a replacement origin for these rocks. Though museum specimens of granites

appear homogeneous, they are far from this in mass; not the least important function of public houses, banks and other opulent edifices, is to display large polished slabs so that geologists can satisfy themselves of this fact. Many demonstrations of the replacement origin of granites and granitic migmatites are available; I need only cite as examples the Cassia batholith of Idaho which, according to A. L. Anderson (1934), has become three times as large by replacement of its bordering country-rocks; or the beautiful replacement phenomena seen in the Southern Greenland rapakiwi granite described by Wegmann (1938). In the case of the Dartmoor granite, Brammall (1932) considers that the aureole has lost a contact-metamorphic zone of higher grade.

Replacement in sedimentary rocks will be controlled by the original structures of such rocks. Permeation by the emanations or ichors will take place preferentially along certain layers depending upon both the chemical and physical nature of these layers. Original platy minerals lying in the bedding planes will become enlarged if they suit the new chemical environment and new-formed minerals will grow so that their direction of greatest crystallization-velocity agrees with the old planes of weakness. The original sedimentary structure may thus become preserved until a high degree of granitization has been reached and the bulk-composition of the rock has been greatly changed. I consider the foliation of granitic gneisses, for example, as essentially controlled by previously existing sedimentary structures, and not by any process of dynamic metamorphism or piezo-crystallization.

Whether the granitizing solutions are thought of as emanations of no specified ancestry, or as ichors from a granitic magma-body, opinion is agreed amongst workers in this subject that they are highly mobile and capable of great chemical action. Working in conjunction with the pore-fluids of the country-rock, they can transform vast portions of the crust. Nockolds (1933), dealing with the contamination of granite magmas, has concluded that the volatiles there concerned form a medium of low viscosity in which diffusion of the reactive materials can take place with comparative freedom. Fenner and others have emphasized the importance of gas-activity in granitization. Emanations of this type, derived from a granitic hearth, move into the walls and, since they carry great supplies of heat, are able to travel long

distances. Even when the activity of one particular batch declines, new accessions of heat arrive from the magma and continue the work. The classic views put forward by Lacroix regarding the efficacy of reactive volatiles must not be overlooked. Gases are able to penetrate where liquids could not. Fenner (1933) points out that in this gaseous transfer there is a strong tendency to reproduce in the contact rocks the same minerals that are crystallizing from the magma. In connexion with direct granitic invasion, therefore, gases may be of great importance; when they have condensed to solutions and have received assistance from the true liquid residuals and from the pore-solutions, they must be capable of gigantic results.

I have mentioned, in a previous paragraph, Milch's preference in considering that the felspars in rocks believed to have been granitized had been derived from the rocks themselves rather than from outside sources. This objection to felspathization by the metasomatic action of introduced materials has been raised by Harker (1932), Thomas and Campbell Smith (1932), and others in Britain. I agree that at certain stages of thermal metamorphism without transfer of material felspars are formed, but such occurrences are not like those of the 'augen-gneisses' and porphyroblast-schists of the great migmatite areas. In felspathization, material for the formation of felspar may be contributed by both the country-rock and the pervasive introduced solutions. There is an abundance of both field and chemical evidence which demonstrates the validity of felspathization. One of the neatest unequivocal cases is provided by Miss Reynolds (1936*a*) in her account of the felspathization of quartzite xenoliths in the Colonsay hornblendite. Malcolm MacGregor and Gilbert Wilson (1939) have dealt with some of the chemical data available, drawing this from such diverse fields as Stavanger, Sutherland, Nevada, Lake Huron, Yugoslavia and Galloway. The field-evidence is especially strong. Beginning with the classic observations of the French school, especially Barrois's beautiful felspar trails at the contacts of the Rostrenon granite, and remembering Grenville Cole's statement that in Donegal 'the schists become porphyritically set with the constituents of the granite', we pass on to a multitude of modern observations showing the validity of felspathization. I list but a few of these in order to bring the weight of the evidence home to

the British; Grout (1937) has listed a dozen examples; and we have in addition the observations of Agar, Barbour and Fettke in the Eastern States, Goodspeed at Cornucopia and Anderson at Inyo in the Western States, Goldschmidt at Stavanger, Read in Aberdeenshire and Sutherland, Du Reitz in Sweden, Wegmann in Greenland, Kranck in Finland, Barth in Norway, Barth in New York, Turner in New Zealand, Alderman in Australia, many examples from the Alps, and dozens elsewhere. The progress of felspathization produces porphyritic gneisses and granites without the aid of dynamic metamorphism. Further, unless we assume the possibility that minutely identical felspars can form in two quite different environments, namely, those of a granitic melt and of a solid country-rock, then the replacement-origin of such granites as those of Shap, Skaw (Unst), and some rapakiwis must be considered as reasonable. But these are topics rather outside my text. All I wish to emphasize here is that granitization and felspathization are valid processes and that they are essentially based upon replacement.

In regions of granitization there has been an afflux of material, either from a 'magmatic' body or from some unspecified source. It is reasonable to believe that during the resulting replacement there occurs an emigration of material. Holmes (1937) has put the whole matter graphically—'the "granite" is the balance of what was there originally, *plus* what has migrated in, *minus* what has been driven out'. The emigrating material moves into the country-rocks adjacent to the theatre of granitization.

The material thus moving through the country-rocks is of several origins and qualities. It consists of (i) the material expelled from the region of granitization, (ii) the direct emanations from the granitizing agent, and (iii) the pore-fluids of the country-rocks. Its diverse constituents must move with different speeds, perhaps depending, as Backlund has ingeniously suggested, upon the ionic radii of the participating elements. Various overlapping belts of precipitation may come into being, and from each a further expulsion may take place. By some such processes zones of various characters arise about the granitization or migmatite core. What seem to be special examples of the operation of this mechanism are provided by the following mainly Scandinavian observations.

Magnusson has deduced at Kantorp, Sweden, what I may call a

precipitation-front of aluminium, iron and magnesia-rich material and a corresponding removal-front of silica, alkalis and lime; the same observer (1938) has interpreted the cordierite-rich Södermanland gneisses as due to a regional magnesia metasomatism connected with the Malingsbo granite. Barth (1938) has described a regional soda-metasomatism in the sparagmites of South Norway, these showing an increase of soda and alumina from an original sandstone to a granulitic gneiss. We may recall too Eskola's (1914) classic work on the Orijärvi area of Finland, where metasomatic replacement of lime and alkalies by iron oxides and magnesia is demonstrated. Wegmann has elaborated a magnesia-metasomatism dependent upon the expulsion mechanism in granitization; he expects to find a zone characterized by cordierite in close proximity to the migmatite area, whilst farther away would be found lower-temperature minerals like andalusite, kyanite, garnet, &c., and farther still tourmaline. The formation of 'fronts' of more or less marked chemical individuality can be exemplified by the soda-fronts of Stavanger, Sutherland, and Cromar, Aberdeenshire, by the potash-front of Hangö, Finland, and by the magnesia-front of Orijärvi; there is no need to elaborate these examples.

From the reference I have just made to Wegmann's views, it is clear that he, with many others, considers the domain of regional metamorphism to be transitional to and genetically connected with the domain of migmatization. I can recall now Termier's saying—no one could reasonably doubt that in the formation of the true crystalline schists there had been an afflux of elements from the depths which had chased the old elements before them. I may recall, too, references I have made in the earlier part of this address to the opinions of Michael-Lévy, Weinschenk, Koenigsberger, Sederholm, Adams, Barrow and others on the genetic connexion between regional metamorphism and granitic intrusion. Before I deal with this final topic, however, it is almost necessary, in view of what I have said in the last few pages, to re-define the title of this address. Igneous action, according to some, may not be concerned in migmatization. As I do not propose to examine here what igneous connotes, I now limit my subject at this late stage to that of the relation between regional metamorphism and migmatization.

One of the most firmly established facts of metamorphic geology is the close association in the field of highest-grade metamorphic rocks and migmatites. I could cite dozens of examples of rocks containing sillimanite and cordierite which occur as more or less discrete portions of migmatitic complexes. I content myself with the mention from our own country of Barrow's sillimanite zone itself in Aberdeenshire, of the cordierite-sillimanite-gneisses of Buchan, and of the sillimanite-gneisses of the injection-complexes of Sutherland and Morar, and of Anglesey—all these high-grade rocks are in or adjacent to areas of migmatization.

The significance of this coincidence has been differently interpreted. Termier, as we have seen, considered that regional metamorphism and igneous activity were two effects of the same cause, the rise of the *colonnes filtrantes*—the emanations of today; the igneous magmas were generated in place. Harker and most British geologists regard igneous intrusion in the sillimanite zone as an incident in the rise of the isotherms in that region. This rise is a direct invasion of the earth's internal heat, and any solutions concerned in regional metamorphism are for the most part not of magmatic origin. As we have already noted, Barrow considered his zones to be in the nature of gigantic thermal aureoles around intrusions of the Older Granite. Barrow's view is that generally accepted by students of migmatization—it is inherent in the interpretations of migmatization and granitization advanced by the Scandinavians, the French and many Americans. I prefer it myself on various grounds. The postulated rise of the isotherms seems a more mysterious process than the bringing-in of heat by magma or emanations; vast supplies of heat and material are necessary in the granitization process and the higher grades of regional metamorphism, and both can be supplied by igneous invasions. Further, there are cases of migmatites and rocks of the sillimanite zone formed above lower-grade rocks—emanations or magma can be injected in such a position, but hardly the isotherms. On these and other counts, I adopt the view of the company familiar with migmatites and see with them a direct causal relation between the highest grade of regional metamorphism and migmatization.

In my opinion, therefore, the cordierite and sillimanite zones are genetically related to granitization. But these zones are the

final stages of the apparently continuous progressive series of Barrovian zones which as a whole supply the common types of regionally metamorphosed rocks. Any departure from isochemical conditions in this series, such as I have suggested in a previous page, may arise through the operation of the advancing fronts of various metasomatisms. I may here call attention to the data, assembled by Brammall (1933), showing that soda increases relatively to potash in the series shale, phyllite, mica-schist, and to comparable observations at many granitization margins. An excellent case is that described by Gilbert Wilson (1938) from Kapaonik, Yugoslavia. Further, argillaceous rocks when involved in contact metamorphism show a change in composition; the rocks adjacent to enormous granitization regions must be more vitally changed. The physical reality of the series of progressive zones is based upon a considerable number of observations in all parts of the world. I need only refer to the work of Barrow, Barth, Balk, Billings, Goldschmidt. T. Vogt, Tilley, Du Reitz, for examples. It seems reasonable to me, therefore, to believe that regional metamorphism as a whole is genetically related to 'igneous' activity of some kind. This is no new belief; it dates from the beginning of our science, and has been held by many, as I have already recorded. A stimulating expression of it was given by Barrell in 1921, and it is inherent in the work of Wegmann and many others.

Out from the central theatre of granitization there pass waves of metasomatizing solutions, changing in composition and in temperature as they become more distant from the core and promoting thereby the formation of zones of metamorphism about it. Very often a spurious depth-control appears to have operated, since the flow of solutions must be largely towards higher parts of the crust. Such a metasomatic metamorphism accounts for the superposition occasionally observed of high-grade zones on lower-grade, and does away with the infelicities attending the alleged inversion of metamorphic zones. Further, the difficulties that the idea of static or load metamorphism fails to meet are surmounted. Deeply buried sediments remain unmetamorphosed unless igneous material gets access to them. In metasomatic metamorphism, original sedimentary textures can be reasonably preserved, mimetic crystallization can prevail, schistosity and bedding, even

in violently folded strata, can coincide. Finally, all those phenomena which Daly felt to be 'truly inexplicable by pure dynamic metamorphism' are satisfactorily explained.

A continuous series of changes in composition from low-grade regionally metamorphosed rocks to migmatites has been established by many investigations. I need only recall those of Goldschmidt at Stavanger and of Barth and Balk in Dutchess County, New York, as examples. Even in the low-grade rocks changes are perceptible and all must be ascribed to the activity of material from a granitization centre. To move this material and to promote the recrystallizations and replacements that occur in cubic miles of rock, great quantities of solvents are required. It seems to me unlikely that the solvents in action are indigenous to the country-rocks—they are more reasonably to be derived from a granitic source. The investigations of Goranson (1931) on the solubility of water in granite magma and the discussion of the problem by Gilluly (1937) indicate that granite magmas may contain possibly 8 per cent. of water. When the enormous extent of migmatitic granites is considered, it is clear that sufficient water is available to promote the changes seen in the regionally metamorphosed rocks.

Are there any indications in low- to medium-grade rocks, not visibly associated with igneous activity in the field, which point to solutions from magmatic sources having travelled so far from the locus of migmatization? In my opinion, we see such indications in the presence of tourmaline in rocks of all grades. Admittedly, this opinion is one not generally accepted in this country. Goldschmidt and others have shown that argillaceous sediments contain an original boron content, and on this account Tilley, for example, suggests that the tourmaline of the regionally metamorphosed rocks is of this derivation. Others, such as J. F. N. Green and McCallien, interpret the tourmaline as due to the recrystallization of detrital tourmaline deposited with the original sediment. These are pertinent objections, but on balance I prefer to regard most tourmaline in metamorphic rocks as due to impregnations from granitization fluids. Tourmaline occurs not only in pelitic derivatives but in rocks of other compositions. In many examples of high- and medium-grade rocks it is clearly introduced, as shown by its relation to the other minerals present. Williamson

in Glen Shee, Billings in New Hampshire, Emmons and Calkins at Philipsburg, Turner in New Zealand, and a score of others, have contributed observations agreeing with this conclusion. Further, Turner, and Barth in New York State, for example, have noted the abundance of tourmaline in low-grade rocks, and have naturally extended the zone of penetration of boron vapours into such rocks from rocks of higher grade. I believe, therefore, that the ubiquitous tourmaline in regionally metamorphosed rocks is an indicator of the action of 'emanations' throughout all grades.

Whilst I have belittled the role of the dynamic factor in regional metamorphism, it is of course true that in the lower grades it must be of considerable importance. How can this be reconciled with the view that migmatization is the prime cause of regional metamorphism? The reconciliation may be sought, I suggest, in the stresses set up by the increase of volume consequent upon the invasion of the crust by the migmatite front. Relief is obtained in the outer and cooler zones by shearing; in the inner and hotter zones by internal reconstructions. The unity of the zonal series may thus be preserved.

My last topic deals with Barth's (Balk and Barth, 1936) four-fold classification of all rocks into sedimentary, igneous (e.g. basalt), metamorphic and migmatitic. Metamorphic rocks are those which have been recrystallized without essential anatectic or metasomatic alteration, and are typified by hornfelses. Migmatitic rocks are formed by the stewing of previously solidified rocks in liquids of magmatic or palingenetic origin. Barth would consider metamorphism as a metabolism of rock whilst migmatization is a metasomatism. Whether we accept this narrow definition of metamorphism or not depends on our ability to decide how much accession of material has taken place in any given rock. Hornfelses, Barth's typical metamorphic rocks, show, as Brammall, Gilbert Wilson and others have demonstrated, marked changes in composition during metamorphism. I feel that Barth's classification obscures the essential unity of regional metamorphism and migmatization.

I have now come to the end of these somewhat lengthy remarks. I have shown my predilection for dividing all rock transformations into two groups, one those of dislocation metamorphism associated

with dislocations of the crust, and the other those of regional and thermal metamorphism, associated with igneous activity. My remarks, I trust, will receive thorough criticism. I am prepared for this, for, just as things too absurd to be said can yet be sung with perfect propriety, so views too tenuous, unsubstantiated and generalized for ordinary scientific papers can yet appear with some measure of dignity in presidential addresses.

II

MEDITATIONS ON GRANITE

PART ONE

1943

I have been particularly anxious about this subject of granite
James Hutton (1726 to 1797) in *The Theory of the Earth*

I. INTRODUCTION

I invite you today to discuss with me 'this subject of granite' and, if we are spared—to use a Victorianism not without cogency at this present time—I propose to ask you a year hence to consider the closely related sequel, granitization. I should perhaps warn anyone seeking an encyclopaedic statement on granite that he will not find it in these pages. The choice of my title is deliberate—these are meditations, not new contributions to knowledge. They have been put together, I confess, chiefly for my own enjoyment. My meditations for this year run thus: How do the authorities define a granite? What did certain of the masters think about granite? What do the moderns think about granite? It will be found that large parts of the field of inquiry are left untilled whilst small patches have been cultivated with sedulous care. But that is the nature of meditations.

I trust I cause no offence when I venture to suggest that the majority of the members of the Geologists' Association are more competent in the stratigraphical and palaeontological branches of geology than in the petrological and metamorphic, and that few members are 'particularly anxious about this subject of granite'—to use Hutton's phrase. I have therefore deliberately chosen these topics of granite and of granitization in order to

bring to your notice the diverse views on these matters. This diversity may not be realized by geologists who have had no occasion to observe recent developments in petrology, and it may come as a surprise to those nurtured on the orthodox textbooks. I propose to survey today, so far as time permits, the field of the origin of granite with the hope of being rewarded by some measure of disturbance in your minds on this matter. New ideas in any field have nothing to commend them in their newness; a survey of the contemporary scene reinforces this opinion. But it is necessary for us students to consider all ideas, new or old, dealing with our particular fields; we cannot permit an Index to restrain our choice. In this spirit, therefore, we should examine the notions on the origin of granite and if any find favour in our sight, we should declare it.

II. CONTEMPORARY DEFINITIONS OF GRANITE

It will be well at this stage to endeavour to obtain a cross-section of contemporary opinion as to what a granite is or should be—to discover what is, in the words of the logicians, the 'essential difference' of granite. The opinion I consult is that displayed in a selection of modern textbooks. Admittedly, many textbooks are out of date so soon as they are published, but textbooks, though suspect to the specialist, are nevertheless the source of basic authoritative information for the general inquirer. From this survey, we should thus obtain a fair view of orthodox modern opinion and be able to present the 'essential difference' of granite or, at least we hope, such portions as are common to several 'essential differences'. Accordingly, we can proceed to consider the definitions of granite given first by the *New English Dictionary*, the *Encyclopædia Britannica*, and the British textbooks, then by the American textbooks and lastly by a few Continental authorities.

The *New English Dictionary* makes the following statement on granite:

'A granular crystalline rock, consisting essentially of quartz, orthoclase-feldspar and mica, much used in building.'

In the *Encyclopædia Britannica*, 14th edition, 1929, vol. 10, p. 636, granite is stated to be:

'Group name for a family of plutonic and deep-seated acidic igneous rocks characterized essentially by the presence of the minerals quartz, felspar [orthoclase, microcline, perthite, anorthoclase (rarely), acid plagioclase] and some ferromagnesian mineral (mica, amphibole and pyroxene).

'Granites are usually wholly crystalline and possess a typical hypidiomorphic or irregular granular (granitoid) texture—whence the name from *Lat.* granum, a grain.'

Further, in the same work, vol. 17, p. 670, appears:

'A granite arises by the consolidation of a liquid at high temperatures and great pressures.'

In his valuable *Nomenclature of Petrology* (1920) A. Holmes defines granite as:

'A phanerocrystalline rock, consisting essentially of quartz and alkali-felspars with any of the following: biotite, muscovite, and amphiboles and pyroxenes . . .'.

Holmes states that *phanerocrystalline* is:

'a term applied to igneous rocks in which all the crystals of the essential minerals can be distinguished individually by the naked eye'.

In A. Harker's *Petrology for Students* (5th edition, 1919) the plutonic igneous rocks include the granites which are:

'even-grained holocrystalline rocks composed of one or more alkali-felspars, quartz, and some ferro-magnesian mineral, besides accessory constituents. The rocks are generally of medium to rather coarse grain, and the tendency of the crystals as a whole to interfere with one another's free development gives what Rosenbusch styled the hypidiomorphic structure.'

Hatch and Wells, in *The Petrology of the Igneous Rocks* (1937) state:

'The acid plutonites comprise what are commonly known as the "granites" . . . The texture of granite is typically hypidiomorphic-granular, that is, one in which the component minerals are equidimensional, and in the main devoid of crystal faces, each grain having common boundaries with its neighbours.'

Turning to the Americans, we find the authoritative statement of A. Johannsen in his *Descriptive Petrography of the Igneous Rocks* (1931, vol. 1, p. 254) that granite is:

'A plutonic rock composed of quartz, potash-feldspar, less sodic feldspar (oligoclase or andesine) and one or more of the biopyriboles (i.e. biotite, pyroxene, amphibole). The rock is megascopically granular . . .'

Other American definitions, for example, those of Cross, Iddings, Pirsson and Washington and of Clarke, are similar to that of Johannsen. A more extended statement concerning the texture of granites is supplied by Iddings (1909, vol. 1, p. 352):

'The texture is holocrystalline and phanerocrystalline; the fabric is chiefly granular; equigranular in some instances; inequigranular and seriate in others; hiatal and porphyritic in still others.'

A standard German definition is that of Rosenbusch (1923, pp. 104–5). Granite is a holocrystalline, plutonic, eruptive rock, and the family is defined as follows:

'Alle Glieder der Granitfamilie sind bei hypidiomorphkorniger, nicht selten ins Porphyrartige neigender Struktur durch die Mineralkombination Alkalifeldspat- Quartz charakterisiert . . . Die bestimmende Kombination Alkali-feldspat-Quartz wird . . . begleitet von Mineralien der Glimmer-, Amphibol- und Pyroxen-familie.'

As a sample French statement we may instance that of J. de Lapparent as given in his *Leçons de Pétrographie*, 1923, pp. 140–1. Granite is an eruptive rock and

'. . . contient comme éléments essentiels du quartz et des feldspaths alkalins, généralement unis à un troisième élément, mica, amphibole

ou pyroxène, et qu'accompagnent dans un grand nombre de cas des feldspaths calco-sodiques. . . .

'La texture typique du granite est la texture grenue, et généralement le grain de la roche est assez gros pour qu'a l'œil nu l'on puisse en déterminer les éléments. . . .'

From this brief sampling, there emerges a general agreement on granite amongst the standard authorities which may be expressed as follows:

A granite is a deep-seated igneous (or eruptive) rock composed of quartz, alkali-felspar and a ferro-magnesian mineral such as biotite, amphibole or pyroxene, in grains sufficiently large to be distinguished by the naked eye, and possessing a texture produced by the crystals as a whole interfering with one another's free development.

In this statement, which we have to accept as the standard opinion, it will be noted that there still remains a freedom in respect to the adjectives *igneous* and *eruptive*. It will be recalled that the British and American authorities used igneous, the Continental eruptive. We have to inquire whether this is a chance usage, whether the two terms are equivalent or whether there is a duality expressing something of significance, and what these terms themselves mean. These are matters which are by no means so trivial as may appear at first sight, and we shall find their discussion will lead us to the very heart of the granite question. We may approach this question by way of an examination of the opinions of certain of the founders of our science and continue it by considering their modern developments.

III. HUTTON, LYELL, CLIFTON WARD, A. H. GREEN

So that no anxious nationalist, jealous of his country's honour, may find himself aggrieved, I preface this section of my remarks by a personal statement. I am well aware of the very great contributions that have been made to the development of petrogenic thought by others than the British, and if I deal now particularly with our native masters it is not because of any lack of appreciation of our debt to the whole community of our science. But surely

Hutton and Lyell transcend any national barrier and are become non-national—for I refuse to ascribe to such as these that debased and suspect attribute of international. Besides, the brightness of the fame of de Saussure or of anyone else will endure without the help of my brief candle. And, after all, that I, bred within sight of Mater Angliae, should deal reverently with a couple of Scots, magistral though they be, surely indicates that I am not without some small measure of grace.

There is no need here to trace the course of the classic strife in the late eighteenth century between the Neptunists, who taught that granite for example was a chemical precipitate, and the Plutonists, who held that granite solidified from a melt. Readers desiring a pleasant and refreshing account of the characters and contributions of the two leading Scottish antagonists in the debate —Jameson the Neptunist and James Hutton the greatest of all Plutonists—will find it in the chapter on Geology by E. B. Bailey and D. Tait in *Edinburgh's Place in Scientific Progress*, 1921. The Neptunist position may be given in the words of Lyell (1875, vol. 1, p. 138): 'According to the original Neptunian theory, all the crystalline formations were precipitated from a universal menstruum or chaotic fluid antecedently to the creation of animals and plants, the unstratified granite having been first thrown down so as to serve as a floor or foundation on which gneiss and other stratified rocks might repose.' Hutton, on the other hand, 'began to teach that granite as well as trap was of igneous origin and had at various periods intruded itself in a fluid state into different parts of the earth's crust. (Lyell 1838, p. 268.) Before I consider such parts of the Huttonian Theory as concern my present theme, I implore geologists to read, or re-read, Hutton's eternal masterpiece, *The Theory of the Earth*; they may find it hard and uphill going, but the effort will reveal unexpected vistas of a profound and genial spirit.

The Huttonian Doctrine is this: 'That, whatever may have been the operation of dissolving water, and the chymical action of it upon the materials accumulated at the bottom of the sea, the general solidity of that mass of earth, and the placing of it in the atmosphere above the surface of the sea, has been the immediate operation of fire or heat melting and expanding bodies.' (Hutton, 1795, vol. ii, p. 556.)

Hutton held that for the 'congelation' of the loose land-derived materials deposited on the sea-floor, a fluidity was necessary—a fluidity which could be produced only by heat or by a solvent, that is, by fire or by water. Water was not adequate as a means of consolidation of strata, whereas it could be demonstrated that 'all the minerals of the earth' had once been in a state of fusion (op. cit., vol. i, pp. 34–49). Besides, 'foreign matter may be introduced into the open structure of strata, in the form of steam or exhalation, as well as in the fluid state of fusion' (op. cit., vol. i, pp. 49–50). Among the 'fossils' demonstrating this fusion, Hutton deals with flint, silicified wood, Carboniferous ironstones, agates, and oolitic limestone (op. cit., vol. i, pp. 59–103). The formation of mineral ores and gangue is explained by a 'sufficient intensity of subterraneous fire or heat, and a sufficient degree of compression upon those bodies, which are to be subjected to that violent heat, without calcination or change' (op. cit., vol. i, pp. 64–6). In the midst of these 'proofs', Hutton sapiently remarks: 'The place of mineral operations is not on the surface of the earth; and we are not to limit nature with our imbecility, or estimate the powers of nature by the measure of our own' (op. cit., vol. i, p. 94).

Having considered the 'concretion of particular bodies, and the general consolidation of strata' and 'those bodies which have been thus proved to be consolidated by means of fusion' Hutton passes on to deal with granite, 'a mass which is not generally stratified, and which, being a body perfectly solid, and forming some part of the structure of the earth, deserves to be considered' (op. cit., vol. i, p. 104). By examination of a graphic granite from Portsoy, Banffshire, he demonstrates that 'the sparry [i.e. felspar] and siliceous [i.e. quartz] substances had been mixed together in the fluid state' (p. 106), and that the rock had been formed by 'congelation from a fluid state' (p. 107). He concludes: 'Upon the whole, therefore, whether we shall consider granite as a stratum or as an irregular mass, whether as a collection of several materials, or as a separation of substances which had been mixed, there is sufficient evidence of this body having been consolidated by means of fusion, and in no other way' (p. 109).

The elevation of the land is due to 'the same agent which had been employed in consolidating the strata—matter actuated by extreme heat . . . expanding with amazing force . . .; some of the

expanded matter might be found condensed in the bodies which have been heated by that *igneous* vapour' (op. cit., pp. 129–30) and so form mineral veins.

This use of the term *igneous* (my italics, above and below) is consistent with Hutton's use of *fire* and *burning*. Fire is responsible for the fluidity of lava and of marble (op. cit., p. 142), a volcano is a burning mountain (p. 149), 'not made on purpose to frighten superstitious people into fits of piety and devotion, nor to overwhelm devoted cities with destruction; a volcano should be considered as a spiracle to the subterranean *furnace*, in order to prevent the unnecessary elevation of land, and fatal effects of earthquakes' (p. 146).

Since granites vary in their composition and texture Hutton considers that they cannot be 'in the original state of [their] creation, since nature would be considered as having operated in an infinite diversity of ways, without that order and wisdom which we find in all her works; for here would be change without a principle, and variety without a purpose' (op. cit., p. 313)—a passage which gives us an epitome of Hutton's view of the flawless character of the earth-machine.

Granite, therefore, cannot have any right of priority (i.e. in time) over, for example, schistus (op. cit., p. 314), and has been formed 'on the same principle with that of any other consolidated stratum; so far as the collection of different materials, and the subsequent fusion of the compound mass, are necessary operations in the preparation of all the solid masses of the earth' (p. 319). Some of the heresies of today appear to have had a most respectable parentage.

Hutton is fortified in this conclusion by the following chain of evidence: 'Proper lavas' or 'erupted lavas' (p. 156) have clearly been in a melted state: whinstone dykes and sheets are just as clearly 'unerupted' (op cit., vol. ii, p. 508) or 'subterraneous lavas' (op. cit., vol. i, p. 154); 'whinstone, trap or basaltes (are) found at last to gradulate into granite' (p. 316), whilst granite also graduates into porphyry (p. 317). From these phenomena it may reasonably be deduced that:

'Granite and whinstone, or basaltes, though distinct compositions, thus graduating into each other; and whinstone, as well as porphyry,

being without doubt a species of lava, we may consider the granite which is found in mass without stratification, in like manner as we do the masses of whinstone, basaltes or Swedish trap, as having flowed in the bowels of the earth, and thus been produced by the change of place, without any proper form of its own, or in an irregular shape and construction' (op. cit., p. 317).

This conclusion that granite 'might be considered as analogous to basaltes, or subterraneous lava, in having been made to flow' (op. cit., vol. iii, p. 13) was confirmed especially convincingly by the relations of granite to limestone in Glen Tilt where granite offshoots clearly veined the country-rocks. Porphyry 'interjections' (op. cit., vol. iii, p. 21) were also observed by Hutton, and he describes dykes of 'whinstone trap and basaltes' in the Midland Valley of Scotland—the last-named rock 'basaltes' is defined as 'a species of granite containing in general iron-ore' (op. cit., vol. iii, pp. 24–5). Comparing these occurrences with dykes and sills of granite and porphyry which he had observed in the Highlands, Hutton deduces that these latter rocks 'appear to have been in as fluid a state as the basaltes' (pp. 24–5). After an account of his observations on the Galloway granite contacts, he concludes: 'Without seeing granite actually in a fluid state, we have every demonstration possible of this fact: that is to say, of granite having been forced to flow, in a state of fusion, among strata broken by a subterraneous force . . .' (op. cit., vol. iii, p. 60).

The validity of a great many of these conclusions of Hutton's is, of course, of a low order, but it must be remembered that we are examining only a small facet of his genius. As Bailey and Tait have remarked: 'Time, which has sadly sapped the foundations, has left much of the superstructure of his system intact.' So far as our present inquiry is concerned, we may observe that Hutton had grouped granite, porphyry, whinstone, trap and basalt as one category of things and had interpreted them as products of the consolidation of material made fluid by heat.

It may be noted that igneous is an adjective only rarely used by Hutton. An examination of the first edition of Lyell's *Principles* reveals, on the other hand, that by the 1830's igneous had become a favoured term. Lyell (1830, vol. i, p. 312) divides the igneous agents into the volcano and the earthquake and in his description

of volcanoes he employs in innumerable cases the particularly igneous qualities of fire, burning, flames and smoke: he quotes the lines, close-packed with combustible ideas, of the Roman poet:

> 'Nec quae sulfureis ardet fornacibus Aetna
> Ignea semper erit, neque enim fuit ignea semper.'

Lyell concludes that the igneous rocks are 'the undoubted products of fire' (1833, vol. iii, p. 4).

These usages of 'fiery' terms are continued even into the 12th edition (1875) of the *Principles*, but so early as 1835 Lyell had introduced a caveat, for he then interpolates: 'When the expressions "flame" and "smoke" are used in describing volcanic appearances they must generally be understood in a figurative sense' (1835, vol. ii, p. 137).

Lyell's early treatment of granite is of much interest for our present purpose. Before entering upon the dark and nebulous territory of the Primary Rocks, with which granite is included, Lyell is at pains to survey once more the sure ground of the other two great classes of rocks; his statements on these are well worth pondering:

> Besides these strata of aqueous origin, we find other rocks which are immediately recognized to be the products of fire, from their exact resemblances to those which have been produced in modern times by volcanoes, and thus we immediately establish two distinct orders of mineral masses comprising the crust of the globe—the sedimentary and volcanic.' (1833, vol. iii. p. 10.)

Again, Lyell (1838, pp. 4–5) proposed four great classes of rocks—aqueous, volcanic, plutonic, and metamorphic; 'the first two divisions, which will at once be understood as natural, are the aqueous and volcanic, or the products of watery and those of igneous action'. I would direct attention to a more or less clearly expressed implication that the igneous rocks are the lavas and the dykes closely associated with them (cf. op. cit., pp. 11–3).

The Primary Rocks contain the metamorphic and the plutonic divisions of Lyell's fourfold classification. The unstratified Primary Rocks are the plutonic, 'composed in great measure of granite, and rocks closely allied to granite' and are 'formed by

igneous action at great depths' (1833, vol. iii, p. 353). The evidence on which this extension of the igneous group is established may be examined in a little detail.

In the first place, veins and dykes of granite penetrate the country-rocks, as so well described by Hutton, MacCulloch and many others; similarly veins and dykes of lava, on Etna for example (Lyell, 1838, p. 12), clearly form 'the channels which gave passage to the lava currents' (Lyell 1875, vol. ii, p. 16). Such dykes 'probably unite with other masses of igneous rock at a greater depth' (Lyell, 1838, p. 172). Both granite and lava (i.e. dyke-material) cause the same changes in the surrounding country-rocks (op. cit., p. 15). Both types of rock have therefore been formed in the same way—by solidification of a hot melt.

In the second place, innumerable passages are known from granite 'into different varieties of rocks decidedly volcanic; so that if the latter are of igneous origin, it is scarcely possible to refuse to admit that the granites are so likewise' (op. cit., p. 15). Amongst the cases of such transitions cited by Lyell are those given by MacCulloch from Aberdeenshire and Shetland, where granite is seen to pass into basalt 'in an equally perfect manner' (op. cit., p. 204).

Lyell concludes that granites, being more crystalline and showing no tuffs, breccias or vesicles, are clearly formed under 'the great pressures of a superincumbent mass, and exclusion from contact with air and water' (Lyell, 1833, vol. iii, p. 364). Granites are therefore plutonic (Lyell, 1838, p. 16). The plutonic rocks are exactly what should be expected by the crystallization of fused volcanic matter at great depths. The relief of the earth's surface, however, is not sufficiently great to exhibit the whole transition from volcanic to plutonic rock, but it is shown in two parts, porous lava to trap, and trap to granite (op. cit., p. 217).

In connexion with the origin of the granitic melt certain early views of Lyell are worth recalling, since they contain the germs of some of the most modern proposals. Isolated patches of granite in country-rock are possibly due to the presence of 'spots in the midst of the invaded strata, in which was an assemblage of materials more fusible than the rest, or more fitted to combine into some form of granite' (op. cit., p. 213). Further, 'if unaltered sedimentary strata contained here and there layers or nests of the

ingredients of granite, the rest of the mass consisting of different materials, and if the temperature of the whole has been sufficiently raised by plutonic action, the result might be that nodules and threads of granite might be formed in certain spots only' (Lyell, 1835, vol. iv, p. 385). The production of greater bodies of granitic melt is also a consequence of plutonic activity. This plutonic influence has sometimes been 'on so grand a scale, [that] we must not consider that the strata have always assumed their crystalline or altered texture in consequence of the proximity of granite, but rather that granite itself, as well as the altered strata, have derived their crystalline texture from plutonic agency' (Lyell, 1838, p. 19). This '*metamorphic theory*' is best stated in the 12th edition of the *Principles*, 1875, p. 139:

'The transmutation has been effected by the influence of subterranean heat acting under great pressure, and aided by thermal water or steam and other gases permeating the porous rocks, and giving rise to various chemical decompositions and new combinations, the whole of which action has been termed "plutonic", as expressing in one word all the modifying causes brought into play at great depths and under conditions never exemplified at the surface. To this plutonic action the fusion of granite itself in the bowels of the earth as well as the development of the metamorphic texture in sedimentary strata may be attributed.'

It is not without interest to inquire how much of Lyell's early position with regard to granite, as given in the 1838 *Elements*, appears to have been abandoned, as judged by omissions, in the 1871 *Student's Elements*. The chief of these omissions appear to be: many statements dealing with passages from granite to volcanic rocks, from granite to trap, and from plutonic to volcanic rocks: the examples used as demonstrations of such passages: the statements that plutonic agency gave rise to both granitic and metamorphic rocks. Lyell regrets that he 'cannot . . . pretend to give in a few words an intelligible account of the long chain of facts and reasonings from which geologists have been led to infer the nature of the [plutonic] rocks', but the result is that 'all granites . . . are supposed to be of igneous or aqueo-igneous origin, formed under great pressures, at considerable depths' (Lyell, 1871, p. 7). I venture to call attention to that word *supposed* in

Lyell's latter statement. Further, my last quotation from this master is this: 'Such facts [i.e. certain concerning hydrothermal action] may incline us to consider whether many granites and other rocks of that class may not sometimes represent merely the extreme of a similar slow metamorphism' (op. cit., p. 570).

So far as I dare interpret the minds of these two great masters, Hutton and Lyell, there seems to be no warrant derivable from their writings for the modern attitude—especially prevalent among those who, to use Hutton's prophetic words, 'judge of the great operations of the mineral kingdom, from having kindled a fire, and looked into the bottom of a little crucible'—the attitude that if one doubts that granites are necessarily genetically related to basalts then one is either mentally deficient, intellectually dishonest or unpardonably iconoclastic. The masters had placed granite and basalt in the same class of things, but they had not derived one from the other. The implications, *to them*, of the term *igneous* must be gauged from their own writings and not from any present-day definition of the word.

The production of granite *in situ* by metamorphic processes—an idea as we have seen certainly not frowned upon by Lyell—was advocated by what I may call a Lake District school headed by Clifton Ward and A. H. Green. In his studies of the Lake District granites Ward (1875, 1876) propounds ideas which, tricked out with a little modern terminology and further spiced with a dash of physico-chemical jargon, would find considerable favour with the most revolutionary of today. Take these on the Eskdale and Shap granites: 'the granite was produced, in great measure at all events, by the extreme metamorphism of the volcanic rocks, such as we now see surrounding it on all sides. That it was exclusively due to the melting-down by aqueo-igneous fusion of these rocks I would not venture to assert; in all probability the heated mass below partly ate its way upwards, at the first, to within reach of the volcanic series, the rocks of which were then metamorphosed to such an extent as to assume the granitic form and structure' (Ward, 1875, p. 590). The Skiddaw granite, on the other hand, might not have been formed by the extreme metamorphism of the rocks *immediately* around it, but could have been produced 'at a somewhat greater depth' and had come into higher levels of the crust by 'eating its way upwards' (Ward, 1876, p. 7). Ward

inquires as to how granites could be produced from rocks so dissimilar as Borrowdale Volcanics and Skiddaw Slates: 'a rock of the composition of slate may be converted into granite by undergoing a change in the arrangement of the elements pre-existing in it, *together with* the addition from below of some of those elements which were lacking in the original slate' (loc. cit., p. 30). The mobility of the magma (I deal later with this term) arising by the metamorphism is a point repeatedly emphasized by Ward; for example, 'it is well to remember that a granitic mass formed by metamorphism of rocks *in situ* is almost sure to become an intrusive mass *at some part* . . .' (loc. cit., p. 30). Ward's conclusions concerning 'the principal conditions under which rocks occur with regard to metamorphism' are worth giving at some length. The states are three:

'(1) That state in which *igneous fusion* is the most important or conspicuous element. This is seen in lava flowing from a volcanic vent . . .

'(2) A state of *aqueo-igneous fusion*, occurring at a much greater depth than the last, and reaching only a dull red heat as a maximum. In this state we have every reason to suppose the granitic magma to be which may now be solidifying at various remote spots beneath our feet. The metamorphism due to moist heat under pressure, at a temperature not exceeding 400° C. may show itself in the complete changing of the mineralogical and physical aspects of a rock, and in the production . . . of . . . felspar, augite, &c. Thus, while granite may be solidifying at a certain spot, the rocks all around may become granitoid and porphyritic.

'(3) A state in which the rocks are permeated by water at a considerably lower temperature than 400° C. . . . [with the production of] . . . minor changes innumerable . . .' (loc. cit., p. 32).

I should add that in the discussion following Ward's paper, J. W. Judd entirely disagreed with Ward's findings and maintained that 'the relations were perfectly consistent with a totally different conclusion from that at which [Ward] had arrived' (loc cit., p. 33). Thus the cleavages appear to have been just as well developed at that time as they are at the present.

The position of A. H. Green is plainly indicated by the 'Natural Classification of the Crystalline Rocks', given on p. 445 of his *Physical Geology*, 1882. Three classes are erected: (1) the Volcanic Rocks, (2) the Plutonic Rocks, including many granites and (3) the

Metamorphic Rocks, including some granites. Green remarks: 'There are granites and granites . . . some formed in one way and some in another' (loc. cit., p. 443). Some granites are produced by the metamorphism of rocks *in situ*: among many cases cited by Green as demonstrating this are several from the Southern Uplands of Scotland described (rather unexpectedly) by the brothers Geikie, Archibald and James. Three successive stages of this metamorphic process give rise to three forms of granite—bedded, amorphous and intrusive. In Green's words (loc. cit., pp. 450–1):

'We have found that Granite occurs under three forms. Under the first it still retains traces of bedding or is interstratified with undoubtedly bedded rocks; here there can be little doubt that it is an intensely metamorphosed rock. Under the second form Granite occurs in amorphous masses, which melt away insensibly on all sides into unaltered strata, show no signs of having burst violently through the adjoining beds, but look as if they filled up spaces once occupied by rocks similar to those that surround them. Such appearances are best explained by supposing that portions of the rock-mass, in the heart of which these bosses occur, have been altered into Granite, the metamorphism having been more intense than that which produced the first form of the rock because the bedding is effaced, but yet not energetic enough to cause the Granite to behave irruptively. Under its third form, Granite gives proof of having been forcibly intruded into the rocks among which it occurs, and its irruptive behaviour may reasonably be attributed to an increased degree of energy in the metamorphic process which gives rise to it.'

Again we see in the views of Ward and Green and of many of their contemporaries no compulsion to consider it necessary either to derive one igneous rock from another igneous rock or to deny that granite, for example, might be formed in several different ways. There were granites and granites. Now the date of these free and easy times should be noted; it immediately precedes the rapid development of microscopic petrography at the hands of Rosenbusch and Zirkel in Germany, Teall and Harker in this country, Williams and others in America. Great and intricate systems of rock-classification were then built up. Of course, I do not wish to belittle the work of the pioneer petrographers, but I fear rocks began to occur more and more in cabinet drawers and less and less in the field. Once all the so-called igneous rocks had

found their places in the pigeon-holes of a lithological classification, it was inevitable that genetic interpretations of the *arrangement* should be forthcoming. But rock-classifications are of many different qualities, and in my opinion it is at this stage hazardous to combine the lithological and genetic qualities in one. The usual kind of classification of the igneous rocks given in the text-books is excellent for the arrangement of rock-specimens in cabinets, but I feel, as I attempt to explain below, that a genetic classification would introduce divisional planes which would appear as dark chasms on the neat charts of the textbooks. I have however, no objection to the use of these charts and tables provided in the first place we realize that they are classifications of rock-specimens as such, with all the criteria of classification determinable in the specimen, and in the second place provided we do not allow these lithological classifications to fetter our genetic speculations. These are matters which I present for your consideration at leisure, and I wish now to return to my main topic.

IV. MAGMA AND MIGMA

In my eclectic statement about granite given on an earlier page I included a freedom between igneous and eruptive, and I noted a British-American preference for the former term and a Continental preference for the latter. One of the outstanding exceptions to this rule is seen in the title of S. J. Shand's book, *Eruptive Rocks* (1927). Shand has given at length his reasons for adopting this title and has used in his discussion Clifton Ward's conclusions on the two states of magma which I have already set forth. Shand's conclusion is:

'It follows then, that we should not be justified in calling granite an igneous rock, if by that name we mean a rock "formed by the agency of fire" or by anhydrous fusion, which is the original connotation of the name. If the term "igneous" conveys to its hearers the idea that the rocks to which it is applied were once in the state of anhydrous melts, with temperatures of 1,000° or more, then it is better to throw it over in favour of an alternative adjective such as eruptive or magmatic, which conveys the idea of fluidity without any assertion or implication about temperature' (loc. cit., p. 12).

We have examined the extension of the term igneous to include not only the volcanic and truly 'fiery' rocks, but also any rocks which appear to have 'been made to flow'. Igneous and eruptive have often been used as if they were synonymous terms, but this usage, as Shand has emphasized, is not strictly logical. What, then, does eruptive mean? One of the best definitions of the eruptive rocks is that of de Lapparent, who states that they evoke the idea of eruptivity by their mode of occurrence: in full his definition runs:

'Les roches éruptives sont des entités géologiques qui éveillent, par leur mode de gisement, l'idée d'éruptivité. Dans un grand nombre de cas on les rencontre sans que l'on puisse d'avance prévoir leur présence; elles rompent la continuité des terrains; leur apparition parait soudaine' (de Lapparent, 1923, p. 79).

Eruptivity, then, is a quality of those rocks which have come to be included in the igneous group. We may now pass on to consider some modern definitions of this word *igneous*. In this consideration we shall encounter immediately another word, *magma*, and shall have to inquire in what senses it also is used.

We may begin with the statement of C. E. Tilley in the *Encyclopædia Britannica*, 14th edition, 1929, vol. 17, p. 670:

'All igneous rocks have solidified from a state of liquidity, the liquid that finally consolidates as rock being technically referred to as magma. Rock-magma is a complex silicate solution carrying gases, the most important of which is water. . . .'

The textbook authorities concur. For example:

'*Magma*—a comprehensive term for the molten fluids generated within the earth from which igneous rocks are considered to have been derived by crystallization or other processes of consolidation.' (A. Holmes, 1920, p. 148.)

Again:

'The igneous rocks are those which have solidified from a molten condition.' (G. W. Tyrrell, 1926, p. 12.) '. . . the original molten rock matter is conveniently termed *magma*. Etymologically magma means a

thick, pasty, porridge-like mass and it is, therefore, entirely appropriate as applied to viscous molten material carrying crystals in process of formation.' (Ibid., p. 47.)

The Americans likewise agree: according to A. Johannsen 1931, vol. i, p. 4) the igneous rocks 'owe their origin to volcanic action, that is, they have solidified from molten material either at the surface of the earth or within its crust. . . .' F. F. Grout (1932) is more explicit:

'Igneous rocks are those formed from cooling magma. Magma is a natural liquid in or on the earth, most of it very hot, made up largely of a mutual solution of silicates but with some oxides and sulphides, &c., and nearly always containing water and other gases held in solution by pressure.' (P. 6.) 'Although the term magma primarily denotes a liquid it may well include those cooling fluids in which partial crystallization has occurred, so long as the mass has not developed notable rigidity.' (P. 143.)

Rosenbusch (1923, p. 27) has this to say of magma:

'Die Eruptivgesteine sind zu geologischer Gestaltung gelangte Teile des schmelzflüssigen Erdinnern. . . . Die Gesteinslehre . . . beschäftigte sich mit diesen Schmelzflüssen nur so weit, als sich aus ihnen die Eruptivgesteine entwickeln und nennt sie Magmen oder Eruptiv-magmen.'

And lastly we have the more elaborate statement of P. Niggli, who, having written a volume called *Das Magma und seine Produkte* (Leipzig, 1937), may be regarded as the final authority. I quote, in my translation, Niggli's definition of magma from his recent discussion of the granite problem (Niggli, 1942, pp. 38–9).

'Rock magma is a red-hot molecular solution, belonging to the interior of the earth or having its origin there, and forming an extensive and connected geological unit. Magmatic solutions, gases and steam which lead to the formation of accessory mineral deposits are closely related with such rock-magmas. In volcanic eruptions magmas flow out as lavas. From this we can infer that the magmatic melt has a complex composition and that both non-volatile and volatile constituents are dissolved in it.'

In the somewhat heated discussion of various heresies which enliven the pages of the pontifical paper just cited, Niggli inadvertently supplies further elaborations of his definition of magma which assist us in our present inquiry: I give them in very free translations: The idea of magma is influenced by the existence of lava, and magma implies a melt (Schmelzflüss) (op. cit., p. 39): the petrographer must be concerned with the question, melt or solid, which has led to the idea of magma (p. 40): magma is in a completely fluid state or is on the point of passing from this phase into the solid condition (p. 45): to comply with the definition, magma must be a completely fluid melt (Schmelzlosung) or the collected squeezed-out melt-material (Schmelzmaterial) (p. 77).

From the various cosmopolitan statements given above it is agreed that the igneous rocks arise by the consolidation of magma, this being for most a molten fluid, completely liquid according to Niggli, but also, according to others, including material in the condition of a mash due to the presence of crystals already separated from the melt. If rocks are formed in some other way they are not igneous. The latter use above of the term magma is more in keeping with the original employment of the word. For example, Scrope (1872) draws an analogy between a lava at the time of its emission and sugar at a certain stage in its manufacture: 'in both cases the matter is not a homogeneous molecular liquid, such as any melted or completely fused substance, but (according to my view of the nature of lava) a *magma* or composed of crystalline or granular particles to which a certain mobility is given by an interstitial fluid, which is in both cases heated water or steam' (op. cit., p. 121). Scrope reinforces this opinion by comparisons between the textures of lavas and of many kinds of sugar, such as barley sugar, brandy balls, sugar-stick and so forth—but since most of us have only dim recollections of what these delectable confectioneries looked like, we can hardly appreciate the justice of his comparison. In another place, Scrope mentions 'a soft mass or *magma* of granules or imperfect crystals enveloped in a liquid' (p. 45). The same usage is implied in his statement of Scheerer's views on the character of plutonic matter at the time of its intrusion: 'All granite formed at one time a kind of watery paste—"une bouillie aqueuse" or moistened magma . . .' (p. 283). Further, in discussing the different textures of Lipari lavas, Scrope states:

'The only possible explanation of the different texture exhibited by the two classes of rock seems to be that the latter [crystalline, stony types] were not thoroughly fused at the time of their emission, but consisted of a granulated *magma*, the imperfectly formed crystals being lubricated by a more liquid base, and by heated intercalated steam, composing a red-hot granulated paste' (p. 335).

The physical state of the broth, mash, bouillie or Brei that Scrope had in mind could theoretically arise in various ways. One kind as we have seen would be a mash consisting of magma and of minerals already crystallized from this magma, 'ein Brei, bestehend aus Magma und aus diesen Magma bereits auskristallisierten Mineralien' (Niggli, 1942, p. 41). Complete consolidation of this mash would give an igneous rock—if the product had the specified mineral composition and texture it would be called granite.

Other kinds of mashes might conceivably arise in other ways than by the formation of crystals in an initially completely fluid Niggli magma. For example, a mash might be produced by the persistence of relict solid pieces in a base which had become fluid, or by the breaking-up and strewing-about of solid rock by introduced fluid. Such mashes would appear to be what M. Reinhard (1935) has called *migma*. When mashes such as these consolidate, they do not give rise to igneous rocks, but, in most cases, to mixed rocks or *migmatites*. The migma, if the amount of its liquid portion became great enough, would acquire mobility, would flow and could intrude itself into its surroundings in typical eruptive or intrusive fashion; the products of its consolidation, however, would still not be igneous rocks. But now suppose the relict portions or the solid rock portions completely disappear so that the migma becomes entirely liquid, what then do we call the liquid and what are its products? Reinhard (p. 44) and Niggli (1942, p. 77) answer that the migma has now become magma and consequently the products of its consolidation are igneous rocks. Again, if the migma sponge is squeezed so that the liquid portion is expelled and collects elsewhere as an entity, then the answer is that the expelled liquid is a magma, too. We may look into these answers a little more closely.

We have seen that most definitions of magma include the attribute of molten; molten means reduced to the fluid state by heat. It is believed, however, that practically anhydrous magmas

may pass by gradual transitions into more water-rich magmas, these into magma-rich solutions, these into concentrated hydrothermal solutions and finally into dilute solutions such as those issuing from a hot spring. Divisions in such a continuous series are accordingly arbitrary, but divisions have to be drawn, especially by geologists interested in ore deposits. T. C. Phemister (1925), for example, states that 'the molten character of a magma is perhaps its most distinguishing feature' and 'if it is proved that . . . aqueous constituents were present to a marked degree, then the term "magma" must be dropped and the phrase "hydrothermal solution", substituted'. Similarly, C. S. Ross (1928) holds that 'magma is rock-forming material in a heat-induced liquid state which on cooling forms an igneous rock'. L. C. Graton (1940) expresses the same view in stating that if 'what crystallizes from the fluid is no longer to be called an igneous rock it may well be that the dominant cause of liquidity has changed from the effect of heat to the effect of solvent'.

In many of the present-day views about the manner of formation of migma-mashes the activity of mobilizers and soakers such as fluids and solutions finds a prominent place, whilst the activity of heat is correspondingly diminished. Is it in order, then, to apply the term igneous rock to the products of the consolidation of migmas which have become entirely fluid magmas without being reduced to this state by heat? We can, I think, use a completely circular argument here which is not such a sophism as it appears at first sight. If the products are like those which we are accustomed to include among the igneous rocks, then the liquid from which they have consolidated can be called magma, however this has been formed. We are, of course, using the argument that a certain rock is igneous because it has consolidated from a magma which is defined as being the fluid which, on consolidation, gives an igneous rock. But we must exclude the deposits from other kinds of solutions such as the saline residues and the vein-stones and vein-deposits generally. As N. L. Bowen (1928, p. 131) has remarked, 'Such liquids [in the two latter cases] are in no sense magmas as the term is ordinarily used, for this usage implies a liquid which, by a single act of injection into a fissure, with subsequent congelation, is capable of filling the fissure with a solid rock.'

As I have already implied, I am of the opinion that we should include in the igneous rocks all those products of the consolidation of a completely fluid magmatic liquid no matter what may have been the ancestry of this liquid. This is in accordance with the best Huttonian and Lyellian practice. It must be again emphasized that the consolidation of a magma, however remarkable and unorthodox the formation of this may have been, gives rise to an igneous rock and not to a migmatite. There is a tendency, as Niggli (1942, p. 77) has pointed out, to regard magmas which have altered their composition by the assimilation of country-rock material as migmas and their crystallization-products as migmatites; this tendency leads to some confusion of thought. I may illustrate what I consider to be the correct usage by reference to my own Scottish experience, though I hasten to state that I shall be somewhat surprised if I have followed my own good advice on all occasions. The matrix in which occur the sedimentogeneous xenoliths of the contaminated rocks of north-east Scotland is an igneous rock though by the act of contamination a normal gabbroic magma has been modified to give on consolidation this somewhat unusual matrix, containing as it does cordierite, garnet and spinel. The orthonorites of the same region are likewise igneous rocks since they have crystallized from a magma though this has reacted with slate xenoliths with the alteration of its original composition. Likewise the lamprophyres of Sutherland are igneous rocks though they are crammed with xenoliths with which reaction has most likely taken place.

It may be noted that I have refrained in the foregoing paragraph from stating what I should call a large piece of contaminated rock, consisting say of multitudes of sedimentogeneous xenoliths in a cordierite-norite matrix or a large piece of xenolith-rich Sutherland lamprophyre. I may at once say that only under extreme pressure would I give them any other names than these. Such pieces are clearly mixed rocks, but before giving them the technical name of migmatite it will be worth devoting a little space to the discussion of this term.

Sederholm has defined his concept *migmatite* as follows: he 'thinks it necessary to use a designation for these hybrid rocks which really characterizes their appearance and origin. They look like mixed rocks, and they originate by the mixture of older rocks

and a later erupted granitic magma, and therefore the name migmatite is the most appropriate' (1926, p. 136). It is important to realize that for Sederholm the concept migmatite was descriptive and genetic—concerned, as he says, with 'appearance and origin'. Migmatites must 'look like mixed rocks', so that the homogeneous permeation-gneisses such as I have described from central Sutherland though formed by mixing, could thus be ruled out; I hope to return to this point—whether migmatites have to *look* like mixed rocks—at a later date. Sederholm's view given in the definition above that a magma, and this granitic, is necessary in the production of migmatites is reiterated throughout his studies of these rocks. For example, a quantity of magma 'arrives from depth' (loc. cit., p. 137), and the proposals of P. J. Holmquist (1921) and others of the Swedish school that the veins in veined gneisses or arterites were mainly exudates from the surrounding rocks arising by ultra-metamorphism are thus wrong. Sederholm prefers to take plutonic magmas as a given fact and to express no opinion about their origin, limiting his study to their relations to the older rocks which they penetrate (Sederholm 1923, p. 4). Again, during migmatization, 'magma intimately penetrates older rock masses' (loc. cit., p. 110) and 'the formation of migmatites depends on the chemical character of the magma'—there being no basic migmatites (loc. cit., p. 147). K. H. Scheumann in his discussion of the nomenclature of migmatites (1937), considers that the provenance of the magma-like or magma-equal portions of a migmatite is not an essential to the definition; this 'magmatic' portion can arise in place and not be connected with a granitic magma. This is a view very different from Sederholm's opinion that every aplitic vein, for example, was connected with some known granite plainly in evidence (e.g., Sederholm 1923, p. 150). We may accept as authoritative the recent statement on this matter from Niggli (1942, pp. 36–7); in a free translation this runs:

'Migmatite should be restricted to rocks and rock-zones which show coarsely variable magmatic and metamorphic structural patterns, originate in the transitional zone between the magmatic and the solid state, and have undergone a metamorphism during which, without any increase in volume, a considerable part attained a fluidal or melted condition. The association of migmatites with a granitic magma is not

necessary to the definition nor do the source and history of the fluid phases affect this either. Mixed rocks like Holmquist's venites can thus be accepted.'

If we went no farther in Niggli's statement we might regard the Scottish xenolith-rich norites and lamprophyres as migmatites. But Niggli proceeds:

'It must expressly be emphasized that rocks with a comparable appearance may be formed which cannot be called migmatites because they did not originate in the border-region liquid: solid within the earth considered as a whole.'

I am accordingly still doubtful about the precise position of my Scottish rocks, but as at least we already know a great deal about their 'appearance and origin' we may leave the discussion of their classificatory status for another occasion.

V. THE GRANITIC MAGMA

We must, I feel, accept this proposition that igneous rocks are those produced by the consolidation of magma, however that magma may have itself arisen; no magma, no igneous rock. If this magma, either as an open or closed system, had the proper composition it would give rise on consolidation to a granite (as defined), and we may call such a magma the granitic magma. I now review some of the proposals which have been advanced concerning the origin of this magma. I must emphasize first that I am here dealing with granitic magma and *not with granitic rock*, and secondly, that whether granitic magmas really exist is no concern of ours at this present.

I may conveniently preface this review by recalling to you the speculations about the nature and arrangement of the earth-shells. That the earth is not uniform from surface to centre is demonstrated by the discrepancy between its density as a planet and the density of the average surface rock, by its shape and by other geophysical lines of reasoning. Study of the vibrations produced by earthquakes is held to indicate that discontinuities exist within the earth which separate zones of different characters—the earth

has a shelled structure. Using the velocities of earthquake waves in the different shells and the observed physical properties of various classes of rocks, geophysicists have made suggestions about the petrographical character of the different earth-shells. R. A. Daly (1933, pp. 173–83) has provided us with a valuable discussion and summary of these suggestions and with those concerning the thicknesses of the several shells. There are considerable divergences of opinion among the geophysicists with regard to both these matters—divergences which should be noted by geologists who, in my opinion, are too apt to become dumb (in all senses of this expressive word) in the presence of the mathematical geophysicist. Daly, after reviewing these divergences and discussing the uncertainties of the observations and deductions, *assumes* (Daly's word, op. cit., p. 177) certain levels as the bases of the earth-shells and makes propositions about their character.

Daly's findings are given in the following scheme:

Layer A: granite dominant; base 30 km. below.
Layer B: piezo-granite (i.e. high-pressure equivalent of granite) passing down into piezo-granodiorite; 40 km. down.
Layer C: piezo-gabbro dominant.
Layer D: vitreous basalt, passing downward, perhaps rapidly, into more ferric vitreous basalt (and then vitreous peridotite?).

Thus is proposed a shell of granitic composition outside a shell of basaltic composition—a finding for which there is strong geological support, as we shall see immediately. Further, from other earthquake data, it is believed that the full sequence is present under the continents, but that the upper granitic layers are thin or absent beneath the oceans.

These proposals are further strengthened by considerations arising from the view that the crust of the earth is balanced, or in isostatic compensation, to use the technical expression. The oceans are deep because the rocks that lie below their floors are heavy, the continents float high because their material is light; we shall therefore never see the heavy layer. On various lines of geophysical inquiry, therefore, it is suggested that the outer shells of the earth consist of light granitic material almost entirely collected in the discontinuous continental masses and a lower

denser continuous layer of basaltic composition. It is now the excellent custom to speak of the continent-making layer as the sial, and the basaltic layer as the sima.

There is no need here to discuss the possible origins of this shelled structure, and I give two views only to illustrate that even in connexion with these deep matters a broad cleavage seen amongst more superficial geologists is already apparent. Daly (op. cit., pp. 322, 422) suggests that the two layers were formed by crystal-fractionation in the outer zone of the primitive liquid earth and that after repeated solidifications, meltings, sinkings and soakings of many transient crusts, the sial and the upper part of the sima have now crystallized to form a true crust. The early thin and continuous sial outer layer has been buckled and thickened into the discontinuous continental masses by Archean mountain-building movements. W. H. Collins (1936) on the other hand has suggested that the sial results from the interaction of the sima and the sedimentary crust, an interaction put into play by mountain-building.

As already mentioned, the notion of a granitic continent-making layer overlying a continuous basaltic layer receives considerable support from the nature and distribution of the visible rocks of the crust. With regard to the upper layer, the investigation of the oldest and deepest visible parts of the crust shows a marked dominance of granitic types—a dominance that is held to increase with depth. From example, J. J. Sederholm (1925) has shown that in Finland the Pre-Cambrian basement-complex is composed of over 50 per cent. granitic rocks together with over 20 per cent. migmatites of granitic parentage, and a similar dominance of granitic rocks is seen in the other large areas of basement-complex in all parts of the world. Daly (op cit., p. 185) has concluded that the main mass of the sial, down to 10 km. at least, has a composition between granite and granodiorite and that below that level it probably approximates to that of granodiorite. With regard to the lower layer, it is a matter of observation that intrusions of basic material have in all geological periods come up from below and penetrated the continental rocks. Further, the oceanic islands are built up dominantly of basaltic rocks, and material of granitic composition is absent from them. Though a slight variation in composition of the eruptible part of the basaltic layer is to be

expected in the course of time, the general similarity of the basic igneous rocks of the continental and oceanic areas alike is reasonably interpreted as an indication that a world-wide basaltic layer is being tapped for their production.

On many lines of geophysical and geological evidence, therefore, it is reasonable to suppose that the granitic sial continents rest upon the continuous basaltic sima below, and that the sial and the upper part at least of the sima are solid and crystallized.

From the early days of petrology it has been clearly recognized that the igneous rocks accessible to our observation belong to two main types, one granitic and the other basaltic in composition. We may present the following data, in illustration of this fact of observation, derived from the summaries by Daly (op cit., pp. 32–41) and T. F. W. Barth (in Barth, Correns, Eskola, 1939, p. 113); from these summaries another fact emerges, namely, the dominance of granite among the plutonic or intrusive igneous rocks and of basalt among the volcanic or effusive types:

Daly:

Granites and granodiorites have more than 20 times the total area of all the other intrusives combined. Basalt and pyroxene-andesite together have at least 50 times the volume of all other extrusives combined.

Barth:

	Gabbroic Composition	Granitic Composition
Extrusive	about 98 per cent.	2 per cent.
Intrusive	5 per cent.	about 95 per cent.

These remarkable facts, recognized as I have said in a general way in the beginning of petrological development, led to the formulation of a group of early views that two magmas, one acid and one basic, were available in the crust and that from these two magmas the great diversity of igneous rocks could be obtained, by mixture or other means. For example, Bunsen in 1851 considered that the variety of the lavas of Iceland could be accounted for by the mixture of an acid and a basic magma both available below that province. The general problem was discussed by Durocher (1857) in a petrological classic (see also S. Haughton,

1862, pp. 1–2). According to Durocher 'toutes les roches ignées, les plus modernes comme les plus anciennes, ont été produites simplement par deux magmas, qui coexistent au-dessous de la croûte solide du globe, et y occupent chacun une position déterminée'. The uppermost magma, acid in character, was lighter and viscous; it rested upon the deeper, denser and more fluid basic magma and was covered by a crust. In the upper magma there was a concentration of volatile constituents, and granites formed from this magma were consequently rich in fluorine- and boron-bearing silicates. Consolidation of the outer part of the acid layer provided the 'granite primitif' so widespread in the oldest parts of the visible crust; basic intrusions were mainly of later date. Durocher suggests the following mechanism for these events: the granitic magma rises along dislocations, helped by compression and by the expansion of the elastic fluids; this proceeding has disturbed the equilibrium of the basic magma below and portions rise into cracks in the solidified granite. In later times the basic magma became more active, but still acid and mixed magmas were available.

For both Durocher and Bunsen, then, granitic magma was available in the crust. Durocher held, however, that this magma was not equally available throughout geological time. As the earth cooled, the outer acid magma solidified more and more, and consequently became less readily injected. Granites were therefore most abundant in the oldest parts of the crust. Certain modern views that there is something special and peculiar about Archean igneous activity are here clearly foreshadowed.

The two-magma view was argued along more modern lines by F. Loewinson-Lessing, the great Russian petrologist (e.g., 1911), who based his conclusions that there are 'two independent primordial magmas', one acid, the other basic, on the following facts and considerations: the mean of two such magmas is the average composition of the igneous rocks of the crust; granitic and gabbroic rocks are abundant, whilst other rocks are developed only in relatively small amounts; granitic and gabbroic rocks each have their own sets of variations, but are not themselves connected by transitional types—in fact, when one type invades the other, hybrid rocks are formed. Before I pass on to discuss the ideas concerning the generation of these magmas I may note that the

existence of a third magma, of peridotitic composition, has been proposed by A. Holmes and others.

Durocher and others held that the acid and basic magmas making the liquid earth-shells were available on tap for the formation of igneous rocks in the outer crust. From geophysical considerations as we have briefly indicated, the existence of these shells as liquid is unlikely and we have now to take up the question of the generation of magma, granitic magma in particular. If we admit that, on other grounds, the two-magma view is reasonable, then we may suggest, at least for discussion, that the acid magma is generated in the sial, the basic in the sima—two magmas, two shells. Apart from the suggestion that liquid pockets might be left as the shells solidified, there are numerous proposals for producing local bodies of magma—proposals utilizing such agents as heat, pressure, friction, reaction, gas-fluxing, radioactivity and so forth—these agents being in many cases put into action by mountain-building processes. I can deal here only incidentally with most of these views.

The doctrine of melting of portions of the crust was, as we have seen, an essential in the Huttonian system. Lyell (1875, vol. ii, p. 201) felt that there was strong evidence to show that 'local variations of temperature have melted one part after another of the earth's crust'. The problem of the possible supply of heat for melting, and especially for pure melting, of the sial and sima is one that today is foremost in petrogenetic discussion. We may briefly consider pure melting first.

Pure melting means the mutual solution of the minerals of the solid rock without the aid of introduced fluids. As Daly (1933, p. 289–90) has recorded, many eminent petrologists have favoured pure melting as a means of producing magma. For example, N. L. Bowen (1928, p. 319) states that 'many granitic magmas may have their immediate origin in the remelting, say by deep burial, of a granite', which according to him was 'derived in more remote times from basic material'—an origin of granitic magma which we shall examine in a later page. Harker (1909, p. 338) remarks that at a deep level in the crust 'extensive melting . . . must be postulated'.

I do not propose here to explore the question of the production of basaltic magma by remelting of the sima and, apart from the

brief catalogue of proposals given above, I shall confine my attention to only two schemes for remelting sialic material to give granitic magma.

In the first place, depression of the sial into the substratum by movements during the initiation and growth of the fold-belts of the crust may lead to its pure melting. The substratum, vitreous or otherwise, could supply sufficient heat for the production of bulky granitic magmas and the heat used could be replenished from the earth's interior. Whilst the base of the great pile of sediments in a geosynclinal depression will not sink low enough for fusion of the sedimentary material to take place during the growth of the geosyncline, the formation of these depressions localizes the belts of folding and hence the belts of further depression and thickening. In time, then, the sedimentary rocks themselves may come within the influence of the heated substratum. There are certain characteristics of the great batholithic masses of granitic composition which have suggested a genetic relationship with mountain-building: for example, their location in the fold-belts, their elongation along the axes of these and their emplacement closely associated in time with the mountain-building (cf. Daly, op. cit., p. 113). According to E. M. Anderson (in Kennedy and Anderson 1938, p. 78) the formation of primary granitic magma is not opposed to any geophysical evidence at present known, but the fusion of the granitic layer can take place only when the layer is thickened by mountain-building movements.

Instead of depressing the sial down into the substratum to be melted, we could consider the substratum to rise into the upper layers, bringing sufficient heat with it to melt important masses of the sial with the formation of granitic magma. It has been shown experimentally by J. W. Greig, E. S. Shepherd and H. E. Merwin (1931) that granite rock melts at a temperature two or three hundred degrees Centigrade below the melting point of basalt. If, for one of many reasons that could be advanced, a large body of basic magma rises from the hot substratum into the sial, it could there melt significant portions of the sialic material. This is a concept that has been discussed especially by A. Holmes (e.g., 1931, p. 241; 1932, p. 543). Holmes emphasizes the essential difference between igneous complexes of the continental and the oceanic areas as shown by the absence of granitic material in the

latter and its abundance in the former; granitic magma appears to be produced where the sial is present to provide the raw material. By generating acid magma through the uprise of basic, there is explained the puzzling association of sharply contrasted acid and basic rocks seen in many igneous complexes. The two magmas of Durocher and Bunsen become placed on tap. Holmes (1932, p. 544) concludes that 'there is a growing belief in the existence of a glassy substratum which is the source of magmas or of a considerable part of the heat necessary to generate magmas from the crystalline materials of the overlying layers'.

We have considered in the foregoing the formation of granitic magma by pure melting of the sial; we may now deal with ideas concerning the partial fusion of the crust and the squeezing out of the resultant liquid from the sponge to form magma. In discussing certain pegmatites found in high-grade Swedish gneisses, P. J. Holmquist (1920, p. 209) referred to the 'real fusion of the most fusible rock masses', suggested that quartz and felspar are the first to liquify, and recalled the earlier proposals of Van Hise, Lane and Daly. This principle of selective re-fusion, combined with the operations of what are called metamorphic diffusion and differentiation, is regarded by many authorities on the ancient rocks as a prime factor in their formation. For our present purpose, however, we may consider the views advanced chiefly by Pentti Eskola (1932*c*, 1933) on the part played by this selective re-fusion in the production of granitic magma.

Eskola states that he has 'become more and more impressed with the idea that granitic magmas must have been formed mainly in connexion with orogenic movements by the pressing out or squeezing of the lowest melting materials, partly from more basic rocks not yet entirely solidified and partly from rocks partly re-fused in the deep regions of the geosynclines' (1932*c*, pp. 456–7). Whilst he 'takes for granted that the primary mode of origin of granites was crystallization differentiation' (loc. cit., p. 459), Eskola finds no evidence for crystal settling in the great Archean granite terrains and therefore prefers a squeezing out of residual magma from a crystal mesh. This granitic magma rises by virtue of its less density or as a result of crustal movements and occupies especially the axial culminations of the folds. Eskola maintains that partial fusion is more plausible than purere-fusion, whilst

sufficient water is present even in the deep-seated rocks to lower considerably the temperature of melting of the lowest melting materials. These would give something more or less like a granitic magma and such magmas may originate from any silicate rocks containing the components of a granite (loc. cit., p. 473).

As noted in the previous paragraph, part of the granitic magma is derived by Eskola by the squeezing out of residual magma from 'basic rocks not yet entirely solidified'—these being, of course, the sima. Several criticisms may be advanced concerning this aspect of Eskola's thesis; one deals with the question as to whether this residual liquid would have a granitic composition—a question mentioned on a later page of these remarks. Another concerns the mechanism of expulsion, and another the quantity of expellable magma. In connexion with this last point, Eskola maintains that 'the amount of sima under the sial is so much greater, and the sima masses mobilized in every geosyncline during an orogenic cycle should be large enough to produce granites on a big scale' (loc. cit., p. 469). This question of quantities of the acid differentiates from basic magmas is one that I return to also in a later page.

Eskola (1933, p. 21) suggests that by this process of selective re-fusion at deeper levels there may very well be developed a highly granitic magma which may be squeezed higher into the crust, there to perform a considerable amount of metasomatism and rock-change of various kinds. These fluids produced by re-fusion are compared by Eskola with those that Sederholm has called ichors, and we may now glance at the generation and function of these.

It will be recalled that Sederholm was of the opinion that granitic magma was available and necessary for migmatization. From such a magma there were derived granitic juices, the ichor, charged with 'emanations' or Suess's 'hot plutonic gases', which were able to penetrate the surrounding country-rocks. These rocks by a process of 're-fusion, although in some cases re-solution would be more appropriate' (Sederholm, 1926, p. 135) were converted into a magma, styled palingenetic or new-born by Sederholm. The process of forming such new magma from solid rocks was called anatexis. The ichor (Sederholm, op. cit., pp. 87–9) is a granitic juice (cf. Eskola, above) graduating between an aqueous solution and a very dilute magma, or it may be a magma containing much

water in a gaseous state. It is able to give a new palingenetic eruptivity to older rocks without changing their composition, but in most cases changes do occur and the resulting palingenetic magma approaches more and more the composition of a granitic magma. Sederholm believed that considerable bodies of granite had been formed by the consolidation of such palingenetic magma; the products of this consolidation are, by definition, igneous rocks —if of the proper mineral composition, they are granites.

There appears to be a tendency now to include within Sederholm's ichors all emanations, mineralizers, soakers, juices, mobilizers, penetrating fluids and so forth, which are considered by some as capable of metasomatically changing a solid rock of another composition into one of granitic composition. It must be realized that just as Sederholm's concept of migmatite has become enlarged so has his concept of ichor. This process of producing a rock of granitic composition is granitization; during it granitic magma may possibly though not necessarily be formed.

I have mentioned in a previous page a view that the mode of origin of Archean granitic magmas, whose products make up so much of the oldest parts of the visible crust, was different from that of the younger granitic magmas intrusive into the Cambrian and later rocks. This view has been elaborated especially by R. A. Daly (1933), who considers that to assume a single mode of origin is to plunge into difficulties. Daly proposes that the Archean granite magmas are formed by anatexis and palingenesis in the newly-formed sialic crust and holds that the production of these voluminous magmas does not directly require the aid of basaltic magma coming from the sima. The conditions of generation of the bulky granitic magmas of Cambrian and later time are essentially different from this. In these, according to Daly, palingenesis is of minor account, and their formation involves the ascent of basic magma from the basaltic substratum. During mountain-building, sialic blocks are depressed into the substratum and, as a result, basaltic magma is injected into the roots of the mountain structure. The blocks of sunken sial are melted by the heat of the substratum with the production of secondary magmas of variable composition. These lighter magmas, especially the granitic, rise through the basaltic layer up into the roots of the mountains and get above the basaltic magma of the initial injection. The granitic magma is

thus in position beneath the thinnest part of the new mountain structure and it is able to make its way by stoping and replacement of its roof to still higher levels in the fold-zone. Thus become established those relationships between the mountain belts and granitic batholiths which I have enumerated on an earlier page. By this mechanism Daly (op. cit., p. 426) considers that he has overcome the difficulties with regard to the supply of heat in the great batholiths. He concludes (p. 446) that there is more than one origin for the world's granitic melts—selective solution during regional metamorphism; re-peated crystallization and re-fusions of initially basic material—to produce both the granites of the primitive sial and some Post-Archean granites; pure melting of old granites; the assimilation of sialic rocks by basaltic magma and subsequent differentiation; and, finally, as a liquid residual from the crystallization of basaltic magma. Mention of this last origin of granitic magma, by the crystallization-differentiation of basaltic magma, leads me to the full discussion of this favoured process.

So far in these remarks, I have had in mind the possibility of at least two magmas, acid and basic, existing or arising in the earth's crust. I have given some account of the proposals which have been advanced at various times to account for their arrangements, relationships and generation. In these proposals there has been inherent the view that granitic magma can be formed independently, so far as parentage is concerned, of any other magma—once the sialic and simatic layers had been established. In other words, we have assumed that the granitic is a primary magma in its own right. This is an early view that suffered a more or less complete eclipse after its formulation by Bunsen, Durocher and others. It was replaced by the hypothesis that the variety of igneous rocks could be derived from one magma by various processes. As a result largely of the laboratory work of N. L. Bowen and others at the Geophysical Laboratory, Washington, faith in these processes has been reduced to faith in one, that of crystallization-differentiation, and the choice of the primary magma reduced also to one, namely basaltic. On this one-magma hypothesis, then, granitic magma is a residue formed by the crystallization of basaltic magma. We may examine this in a little detail.

A complete statement of the origin of the diverse igneous rocks by crystallization-differentiation will be found in the work *The Evolution of the Igneous Rocks* (1928) by N. L. Bowen. Elaborations and amendments of the general theory have been provided by P. Niggli (1938, p. 610), W. Q. Kennedy, S. R. Nockolds and dozens of others. The theory reached its greatest popularity (rather unhappily in my opinion) during the investigation of the Tertiary igneous complexes of the West of Scotland by H.M. Geological Survey and has accordingly left its mark upon the discussions in the Survey Memoirs describing those complexes.

Observation shows that magmas of basaltic composition have been available in vast quantities throughout geological time. The great volume and the uniformity of the basaltic masses in the earth's crust have led to a general agreement among petrologists that basaltic magma is primary, that is, produced independently of other magma. I have already given on a previous page a few data on the abundance of basaltic rocks. On many geological grounds (which, after all, are the best grounds for geologists) we may be sure that primary basaltic magma is available. How it may become available is a matter that does not directly concern us here; we have to examine what is supposed to have happened to it since it arrived in the crust.

The notion of the sinking of early-formed crystals in a magma as it cools and solidifies is one propounded over a century ago by Scope and by Darwin. If these early-formed crystals can be collected or removed in some way, or if the liquid can be squeezed out from a mash of them, then it is obvious that a residual magma may be formed which may be very different in composition from that of the initial magma. In the case of our primary basaltic magma, the early-formed crystals will be of olivine, heavy and rich in iron and magnesia, poor in silica, free from alkalis; their separation from the magma will leave a residual fluid richer in silica and the alkalis—a fluid which, by a continuation of the fractional crystallization process, would *tend towards* a granitic magma. Bowen (1915) has demonstrated that heavy crystals do sink in artificial melts, so that a gravitative separation of early-formed heavy crystals such as olivine may take place in our primary basaltic magma. Further, as was long ago suggested by our own George Barrow, residual magma may be squeezed out of a mash

of such early-formed crystals and provide bodies of liquid that may move away from their place of origin; squeezing may be due either to orogenic causes or simply to the weight of the network of the early crystals. By some such mechanism as this, therefore, it is considered that a diversity of igneous rocks may be produced.

No one would deny that crystallization-differentiation might happen in the laboratory; a large number of eminent petrologists go so far as to affirm vehemently that it has happened on countless occasions in nature. Whether it has there happened or not, can be decided on geological evidence. In this connexion we should constantly remind ourselves of L. C. Graton's statement of our failing when confronted with physico-chemical argument,—we seem 'to suspect that a single chemical or physical fact can completely submerge any number of geological facts' (Graton, 1940, p. 201). I do not propose to discuss the validity of the general theory of crystallization-differentiation and will confine my attention to certain criticisms concerned with the production of granitic magma in quantity by this process.

The first criticism concerns the *quality* of the residual liquid. It has been contended by C. N. Fenner (1929, 1931) of the Geophysical Laboratory, Washington, that 'crystal separation alone, acting upon a basaltic magma is observed to lead to a residual liquid rich in iron'. What appears to be a first-class demonstration of this in the field is provided by the remarkable investigation by L. R. Wager and W. A. Deer of the Skaergaard Intrusion in Greenland (1939). These authors state: 'From the evidence of the Skaergaard intrusion it appears that crystal fractionation of basalt magma leads to the ferro-gabbros and not to intermediate rocks of the calc-alkaline series' (op. cit., p. 335). They consider that calc-alkaline rocks may be the result of the mixing of acid and basic material, either basic magma with sialic crust or acid magma with basic rocks—in any event, 'the amount of ferro-gabbro material present in the known part of the Earth's crust is small; this provides a further indication that the bulk of the Post-Cambrian granites have not been formed by fractional crystallization of basalt magma'. Fenner's contention has been met by Eskola, Niggli and others, mainly on the ground that Fenner's observed enrichment in iron in the residual liquid is small compared with the much larger changes in the other components which tend

towards a granitic composition as a whole. Niggli (1942, p. 72) further somewhat airily dismisses the conclusion of Wager and Deer as being yet another example of a bad practice in scientific investigation—generalization from a special case, unwarranted broadening of the field of application of the results and so forth. I need go no farther in this debate and only wish to direct attention to somewhat divergent opinions on these matters amongst those skilled in physical chemistry.

The second criticism deals with the *quantities* of the products of different compositions resulting from the fractional crystallization of basaltic magma. In a previous page, I have given a summary of the estimated volumes of the different kinds of igneous rocks in the earth's crust—the dominant plutonic rock is granite, the dominant volcanic rock is basalt, intermediate rocks are relatively rare.

It is obvious that from a given volume of basaltic magma only a small quantity of granitic magma can be produced by crystal fractionation whilst the products of intermediate composition must be bulky. Calculations of the amounts of the various products have been made by F. F. Grout (1926) who finds that 'as a maximum one-tenth of an average basaltic magma may be granite'; another calculation by Grout gives the following amounts—from 100 parts of basaltic magma there can arise 50 parts of olivine-gabbro, 30 of gabbro, 10 of diorite, 5 of quartz-monzonite, leaving 5 parts of residual granitic magma. Intermediate rocks should therefore be very abundant and granitic rocks rare—this is not the case in the visible crust. This is a cricitism of the crystallization-differentiation theory which has been emphasized by A. Holmes (1936*b*, pp. 230–1), R. A. Daly (1933, e.g. pp. 401, 425, &c.), T. Krokström, C. N. Fenner and others. Daly points out that on the calculations of Bowen himself, the residual liquid is still basaltic even after half of the original magma has crystallized—a result which, as Daly says, 'affords a lively idea of the stupendous quantity of basalt that would have to crystallize in order to make a batholith of granite by the same process. Is such volume credible in the case of any Post-Archean batholith?' Again, the enormous bodies of parental basaltic magma from which the granites are supposed to be derived have to make way for themselves in a crystallized crust, and, also, their crystal fractionation would give rise to

vast accumulations of heavy material—an accumulation for the existence of which beneath the granite batholiths geodetic observations provide no evidence whatsoever. Whilst the amount of the visible acid volcanic rocks like rhyolite appears to be of the right order to be derived by crystallization-differentiation from the visible basalts, the gigantic volume of granitic rocks is, in my opinion, an unsurmountable objection to this theory. After all, what has to be explained is what is seen in the crust, not what is seen in 'the bottom of a little crucible'.

The adherents of the crystallization-differentiation theory have made different kinds of attempts to answer this objection. For example, N. L. Bowen (1928, pp. 90–1) supposes that the product of crystallization at intermediate stages, say the dioritic, is represented by certain layers of the zoned crystals and not by a separate rock-mass— a proposal which seems to me to have little bearing on the granitic magma. S. R. Nockolds (1936) believes that 'the acid residuum could amount to at least 15 per cent. under favourable circumstances', an amount that appears to me still to be ridiculously small. Eskola, as we have already seen, states that since basaltic magma contains only small quantities of the granitic elements, 'we must assume that the amount of sima under the sial is so much greater' and enough sima should be mobilized in each orogenic cycle to produce granite on a big scale—an argument, of course, which is remarkable, to say the least about it.

The recent polemic of P. Niggli (1942, pp. 51, 52, 62, 69) is still more remarkable on this point: he holds it better to consider the basic and acid magmas as the initial and final stages of one series of developments, the transitional stages of which are revealed only under special circumstances, such as intrusion at the right moment; these intermediate products are usually eroded (op. cit., p. 51). In any case, Niggli proceeds, too much emphasis has been laid on light and dark rocks, acid and basic rocks, so that a completely false impression of a hiatus between the two groups has been given. This emphasis on two magmas, acid and basic, is a scientific superstition (Aberglaube), a reminiscence of a time made obsolete by the advances in field-petrography, microscopy and physical chemistry (op. cit., p. 52). Since the Post-Archean granites are not connected with the Archean granites, they cannot be produced from them by remelting. Granites are so closely connected with

basic rocks in one family that the idea of different origins for acid and basic magmas is absurd—acid and basic magmas are a geological unity (op. cit., p. 52). It must be remembered that only the *visible* proportions of different rocks are seen, whereas crystallization-differentiation acts below, and the granite magma, being lighter and more gaseous, comes to the top (op. cit., p. 62). The dominance of granites among plutonics and of basalt among volcanics is exactly what is to be expected, since granite has had plenty of time to form from basalt, whilst basalt comes into place quickly and remains primary and undifferentiated (op. cit., p. 69, footnote). I feel that I may well leave the unbiassed reader to assess the validity of these protestations of Niggli; when they occur scattered through a lengthy memoir they are apt to be accepted piecemeal—they become rather less palatable when presented in bulk, as I have done.

The third major criticism of the crystallization-differentiation hypothesis is concerned with the *physical mechanism* of the process. Mere crystal settling under gravity is felt by many to be inadequate to separate the residual fluid from the crystal framework. This is a subject discussed especially by A. Holmes (1936*b*). If by the time the residual fluid has arrived at the correct granitic composition it constitutes only 5 per cent., 10 per cent., or even 15 per cent. of the whole mash, it will require very powerful squeezing of the sponge to expel the residual magma to form discrete granitic bodies. No field evidence can be produced to demonstrate the operation of such stresses upon the basic masses. Possibly a larger percentage of residual fluid than those stated above could soak out of the sponge, but then its composition would be by no means granitic.

On these and many other grounds I for one prefer to remain in the benighted state of superstition so heretical to Niggli and the other pontiffs of crystallization-differentiation. I believe that there are fundamental genetic differences between the granites and the basalts which go far beyond any one-magma or two-magma or multi-magma theory. Both groups are igneous rocks, by definition, and both appear in their appropriate pigeon-holes of our beautifully symmetrical classification of the igneous rocks. But once we proceed beyond a classification of rock-specimens as they occur in the drawers of a cabinet, we find, as I foreshadowed in a previous

page, dark chasms opening up on our neat charts. The examination of this aspect of the matter provides my last topic for this year's address.

VI. THE PLUTONIC AND THE VOLCANIC ASSOCIATIONS

As we have already seen, a clear distinction was drawn in the early days of our science between the Primary Rocks and other rocks. The manner of formation of the sedimentary rocks and the eruption and solidification of the volcanic rocks were matters of observation open to the inspection of all. It was not so with the Primary Rocks, whose innermost secrets were not and still are not revealed by a wave of the uniformitarian wand. As the science developed, more and more of the Primary Rocks became attached to the volcanic products to form the igneous rocks, united by one genetic character—consolidation from a magma. I have examined at length the consequences of this union—the chief being the view that since there could not be eight hundred magmas there must clearly have been only one. Some of us are endeavouring to free ourselves from this restraint. It is worth while recalling that Lyell, in my opinion, never really accepted the igneous rock category. In the first edition of the *Principles* (1833, vol. iii, p. 374) rocks are divided into the aqueous and the volcanic, the plutonic and the metamorphic; exactly the same fourfold classification appears in the *Students' Lyell* of 1896 (1st edition, p. 15); the volcanic and the plutonic rocks are in separate compartments. We may remember, too, Green's 'Natural Classification of the Crystalline Rocks' into Volcanic, Plutonic and Metamorphic.

The fundamental difference between volcanic and plutonic rocks has recently been presented in a memoir by W. Q. Kennedy and E. M. Anderson (1938), who, by bringing to a focus a multitude of searchings and gropings of many of us, have made in my opinion one of the greatest contributions of any time to the advancement of petrogenesis.

Kennedy, in the first part of the memoir cited, recognizes two groups of igneous bodies which he calls the volcanic and plutonic associations respectively. It is important to realize that the volcanic association includes also all intrusions which are genetically related

to the volcanic activity and originate in the same magmatic source. The volcanic association is found in non-orogenic areas, its members are overwhelmingly basic in character, and the small amount of acid volcanic material formed may conveniently be explained by crystallization-differentiation of basaltic magma. The plutonic associations, on the other hand, are granitic and granodioritic in composition, are emplaced in close association with orogenic movement and present abundant evidence of processes of assimilation and contamination. Kennedy's conclusions may be stated in his own words (op. cit., pp. 31–2):

'(*a*) *Volcanic* and *plutonic associations* represent two distinct and apparently independent expressions of magmatic activity: they have their origins in different parent magmas and their subsequent evolution is controlled by different processes.

'(*b*) *Volcanic associations* are derived from a universal basaltic magma (or magmas) which originates by remelting of a basaltic earth shell, the intermediate layer.

'(*c*) *Plutonic associations*, despite their more deep-seated location, originate at a higher level and are derived from a primary, universal, granodiorite parent magma. The latter develops by remelting of the so-called "granitic" layer. Such remelting would only be possible within the orogenic zone of the earth where tectonic thickening of the crust brings the base of the granite within the range of melting.

'(*d*) The actual mode of irruption differs in the two cases. The granite and granodiorite batholiths appear to penetrate slowly upwards, accompanied by a wave of granitization and migmatization of the country rocks, until arrested by some unknown form of hydrostatic control before they reach the surface. The ascent of basaltic magma is entirely different, no vast intercrustal reservoirs are formed and the basaltic melt appears to be irrupted directly towards the surface through a system of relatively narrow, dykelike fissures. It is then either extruded in the form of lava flows or else gives rise to larger or smaller injected bodies, such as sills and laccolites, which may themselves represent volcanic reservoirs. Differentiation of the basic magma, moreover, takes place within the levels of crystallization and, in consequence, the magmatic evolution is controlled largely by fractional crystallization.'

Here, then, is a clear statement freeing the granites from their bondage to a parental basaltic magma—a statement in my opinion upon which we may build a proper rearrangement of all the

assorted material that has been thrown into the igneous rock compartment. This rearrangement is a task to which we should address ourselves.

* * *

My meditations for this year are finished. I have myself a clearer idea of what a granite is considered to be, and a better acquaintance with the proposals concerning the making of granitic magma. I and you, I trust, are now more fitted to go forward together a year hence into the mysterious, maybe fabulous, territory of granitization. If these remarks, and those of next year, should be published, I make a plea that the two addresses at some time be read together —they are complementary.

The present remarks were headed by a quotation from Hutton. I close with another:

'While man has to learn, mankind must have different opinions. It is the prerogative of man to form opinions; these indeed are often, commonly I may say, erroneous; but they are commonly corrected and it is thus that truth in general is made to appear.'

III

MEDITATIONS ON GRANITE

PART TWO

1944

I. CONNECTIVE

IN Part One of these meditations presented to you a year ago, I essayed among many other aims to arrive at acceptable statements concerning a number of aspects of the common everyday geological adjective igneous. I trust that you agreed with me, in the first place, that the igneous rocks are those, and only those, that have consolidated from a molten fluid, in the second place that this molten fluid or magma may have been produced in a variety of ways, and lastly that its ancestry does not affect in any way the legitimacy of its products—they are all igneous rocks. We agreed, I continue to trust, that weighty and cosmopolitan authority had pronounced granite to be an igneous rock and accordingly I passed in review the proposals dealing with the origin of granitic magma. We saw that formidable objections had been raised against the derivation of granitic magma through the crystal-fractionation of a primary magma of basaltic composition; we preferred, again I venture to hope, to consider that granitic magma was primary in its own right, and we noted the possibility that the sialic layer of the crust might be its source. Finally, after many buffetings, we fetched up for the year in the haven of the great thought that possibly the igneous rocks might be of two fundamentally different qualities—the volcanic and the plutonic associations. Refitted and refreshed, we now push out on a new Odyssey.

What lies before us today is this. We have first of all to consider

at some length the proposals concerning granitization, that is, broadly speaking, the acquisition of granitic characters by rocks of one kind or another. The validity and scope of granitization processes are questions which now divide petrologists more sharply than any others. This same division has lasted for nearly a century; its causes and results are worth examining. We shall find—and I hope find reasonably—that there is an excellent case to be made out for granitization.

Once this has been admitted, we shall realize that rocks with granitic characters have been formed in more than one way—there are granites and granites; some granitic rocks are of igneous origin, others arise by granitization. We have to examine, however, the possibility of all kinds of granitic rocks, igneous or not, being the consequence of one cause and marking stages in one unified process. We then seek to extend this unification still farther so as to embrace the metamorphic rocks and thus to interpret the whole of the classic group of the Primary Rocks as results of one mechanism. During these deliberations, that ancient and respected term igneous will be found to have become shorn of its importance. But once we have the true measure of the igneous idea, once we realize the amount of extra-belief which has accumulated around its primitive kernel of utility, we shall be prepared for a reorientation of our views on rocks—a reorientation which is simply nothing more, and nothing less, than a return to Lyell.

II. GRANITIZATION: DEFINITIONS

It will be as well to preface the consideration of granitization with an examination of the significance of the term. There is no need here to trace its history and I content myself with a few recent statements from selected diverse sources.

My first definition and discussion come from one of the most important of the modern papers on granitization, that on the Inyo Range by G. H. Anderson (1937, pp. 47, 49); Anderson states:

'Granitization has never been precisely defined. In a narrow sense it should relate to the formation of a granite, but, as, theoretically, rocks

formed by replacement and recrystallization may have the composition of monzonites, granodiorites, diorites or gabbros, the term might well be extended to these. This would avoid such cumbersome terms as "granodioritization" or "gabbroization".

'Used in this sense, granitization is a complex process, involving both recrystallization of sedimentary materials under the influence of elevated temperatures and pressures, and reactions between those materials and emanations introduced, possibly often in comparatively small amounts, from a subjacent magma. Reactions and recrystallization doubtless continue concurrently.

'An attempt should be made to distinguish between assimilation and granitization. In the first of these, magmatic fluids are in excess and dissolve or absorb sedimentary rocks . . . either by "melting in" or reacting with them before crystallization. In the other, the granite is formed in place, by reaction between magmatic fluids and the solid minerals of the sediments. Solution and recrystallization go on concurrently; no large part of the mass is liquid at any given time; most of the magmatic emanations, in fact, may be gaseous. The amount of the material added from the magma may actually be relatively small, so that the process may be only replacement.'

Granitization has been defined by Malcolm MacGregor and Gilbert Wilson (1939, p. 194) as follows:

'We define granitization widely as *the process by which solid rocks are converted to rocks of granitic character;* this includes all such processes as *Palingenesis*, *Syntexis*, *Transfusion*, *Permeation*, *Metasomatism*, *Migmatization*, *Injection*, *Assimilation and Contamination*, as used by various authors, but rarely with well-defined meanings.'

Another general statement, especially valuable since it comes from a geologist not overwhelmingly interested in granitization, can be assembled from a discussion by L. C. Graton (1940, pp. 301–2). The composite statement is:

'The aureoles of granitization, employed in the usual broad sense which does not require that the parent magma or the resulting product necessarily possesses the exact composition of a true granite . . . represent the transformation of all sorts of earlier rocks into material of such granite-like composition and texture that in many instances it has been mistaken for normal intrusive rock.'

In his discussion of the granite problem, P. Niggli (1942, p. 28) stresses that granitization and felspathization are used to cover the products of many different processes, the chief of these being:

(i) Mineralogical and textural changes, by the development of a granular texture with newly formed felspar; pure recrystallization has taken place without change in the bulk composition of the rock.
(ii) The transformation of rocks into 'granite-like' rocks by recrystallization, combined with pneumatolytic-hydrothermal transfer of material as, for example, alkalis.
(iii) Granitization by injection-processes.
(iv) Granitization by ultrametamorphism, as, for example, of sedimentary rocks, combined with melting and transfer of material. Palingenesis is a border-line case of this type of granitization.

It will be an advantage to formulate a definition of granitization as free as possible from genetic controls, speculative and otherwise. Possessing such a broad and acceptable foundation, each of us may build our solid and everlasting edifices, or our ramshackle and ephemeral hovels, on our own particular corners of it. In its broadest and simplest sense, shorn of all genetic limitations, granitization means 'the process by which solid rocks are converted to rocks of granitic character'—to use the words of MacGregor and Wilson. We may meditate on two aspects of this simple statement. First, it implies that there are granites which are not the product of granitization, for there must be a standard granite for comparison which is not of this parentage. The standard granites are presumably those formed by the consolidation of granitic magma—the Granites of Authority, dealt with in my address of last year. As I did then, I again make a plea to you not to regard the existence of such granites as an article of blind faith. Rather, for the purposes of this discussion, let us cautiously assume that igneous granites have been identified as such by authority. It is with granites having this kind of validity that the products of granitization are compared.

Examination of the second aspect of the definition given by MacGregor and Wilson indicates that some genetic limitation is there necessary. Unless we suppose that all magmas are relics of the primitive liquid shells of the early earth, we have to admit that magmas arise by liquefaction of solid rocks. Granites of igneous

origin would therefore comply with the definition. We may recall the statement of Hutton: 'Nature has formed the granite on the same principle with that of any other consolidated stratum; so far as the collection of different materials, and the subsequent fusion of the compact mass, are necessary operations in the preparation of all the solid masses of the earth.' We may remember the various proposals for the production of granitic magma by the melting of the sial. In all these proposals, ancient and modern, solid rocks are converted to rocks of granitic character, but through an intermediate magmatic stage. It is this genetic limitation that has to be added to the definition of granitization given by MacGregor and Wilson so that the field of our present inquiry can be within manageable compass.

I therefore add an amendment and would define granitization thus:

Granitization means the process by which solid rocks are converted to rocks of granitic character without passing through a magmatic stage.

A definition by Grout (1941, p. 1540):

'Granitization includes a group of processes by which a solid rock (without enough liquidity at any time to make it mobile or rheomorphic) is made more like granite than it was before, in minerals, or in texture and structure, or in both,'

is somewhat similar, but has implications which I cannot accept. I propose to inquire now whether granitization has happened or not by examining specimen proposals and debates of differing degrees of relevancy.

III. THE FRENCH SCHOOL

The differences, often profound and constant, between national schools of geological thought would form a study of surpassing interest and entertainment. One branch of this study would be concerned with the cleavage between the French and Germans on what happens at granite contacts. For over a century French geologists have consistently advocated granitization in one form

or another at such contacts. Modern advocates of the process often forget the debt they owe to this great French school; they should 'look unto the rock whence they are hewn, and the hole of the pit whence they are digged'. Accordingly, I display here a selection of the greatest of the French contributions.

As early as 1824 Ami Boué suggested that heat and emanations coming from the interior of the earth were responsible for processes which we should now include in granitization. Boué says (I quote from Niggli, 1942, p. 23):

'La chaleur ignée et les émanations gazeuses de l'intérieur de la terre auraient donné aux schistes peu à peu et sous une plus ou moins forte compression une espèce de liquéfaction ignée. . . . Les éléments des schistes auraient perdu de leur force de cohésion, leurs parties constituantes auraient été écartées les unes des autres, et les émanations gazeuses auraient pu s'insinuer dans les vides ainsi laissés. De cette manière les affinités chimiques auraient pu s'exercer dans certaines limites, posées par les forces adverse de la cohésion, et les parties constituantes des roches auraient pu prendre pendant la liquéfaction et le refroidissement lent un arrangement plus ou moins crystallin suivant les circonstances, et sans déranger ou détruire notablement la structure feuilletée, primitive. De plus, le jeu des affinités chimiques aidé par les substances etrangères. introduites, pour ainsi dire, par sublimation dans ces roches, auraient donné naissance à cette foule d'espèces minérales crystallines, qui sont disséminées en nids, en amas et en petit filons au milieu des schistes crystallins.'

In the eighteen-thirties, felspathization and connected topics were touched upon by the master hand, by no means yet come to its full power, of J. Fournet of Lyons (*Neues Jahrb.*, 1837, 1838; see P. Niggli, 1942, pp. 25–6). Fournet considered that among the metamorphic rocks are some that result from simple recrystallization and others from partial melting, combined on occasion with emigration and immigration of material. He realized that rocks very much alike may be of diverse origins; a gneiss, for example, may be of eruptive origin and come as molten material from deep in the crust, but a very similar rock may arise by the modification of a sediment. He asked the fundamental question: how shall we decide whether this or that gneiss is a molten rock or a clayey rock which has been felspathized and crystallized with

the formation of mica? Back comes the answer at once—an answer that we, an association of geologists, should cherish; it is the geological relations which make our decision much easier. This answer has served as my text for many exhortations during the last few years when I have stressed the overwhelming importance of the *geological* evidence in all problems of granitization, migmatization, contamination and related processes. In a recent discussion at the Geological Society, I propounded what seems to me the trite and obvious doctrine that the strength of cases of this nature is the strength of the geological evidence; you can accept this evidence on faith in the observer or you can make a pilgrimage to see it for yourself—you can follow Desmarest's (I believe) century-old advice to *go and see*. What you must not do, however, is to remain meek when confronted with a non-geological argument that appears to conflict with the geological evidence. It is true that the meek shall inherit the earth, but the earth is already the geologist's.

The next major development in the French body of thought on these matters was the concept of mineralizers, *agents minéralisateurs*, formulated by H. Ste-Claire Deville in 1841. As Deville's original concept has been elaborated and extended during the century of its usefulness—such elaborations and extensions being indeed a measure of this usefulness—it will be as well to present the original definition (1841). It is as follows:

'Parmi les matières gazeuses que nous rencontrons dans la nature, il en est quelques-unes qui, sans se fixer sur aucune des substances qu'elles touchent, les transforment ou les transportent en les transforment en matières minérals, absolument semblables à celles que l'on rencontre dans la nature. . . . Ce sont ces substances que je propose d'appeler *agents minéralisateurs;* je les caractérise par cette perpétuité de leur action que se continue indéfinement, jusqu'à ce qu'elle soit fixée par des matières autres que celles sur lesquelles elles sont appelées à réagir pour ainsi dire par leur seule présence. Ces substances, quand elles existent dans la nature, ce qui permet de les faire entrer dans les hypothèses de la géologie, sont toutes compatibles avec l'eau qu'on rencontre en effet partout, et l'eau n'annule et n'amoindrit jamais leur effet spécial.'

In order to illustrate the next stages in the evolution of the

French ideas I cannot do better than analyse the relevant contributions appearing in the 1847 Bulletin of the Geological Society of France (*Bulletin de la Société Géologique de France*, vol. iv, 2nd Series, 1847). This volume contains contributions of fundamental importance to our present inquiry by such a galaxy as Virlet d'Aoust, Élie de Beaumont, Durocher, Fournet and Scheerer. We may glance briefly at certain aspects of these—I leave the contemplation of the finer points of the swordplay of what developed into a pentagonal duel of some sprightliness to the interested student of French character.

For some twenty years, Virlet d'Aoust had been much concerned, both in Greece and in France, with contact phenomena of an intense kind and with the production of granite and porphyry by metamorphic processes. In 1844 he had presented his *Note sur les roches d'imbibition* (Virlet, 1844), in which he had described in a general way phenomena of soaking and penetration by igneous materials of sedimentary country-rocks. His notions and his terminology have influenced French thought to this day. I take this opportunity of making my *amende* to Virlet's manes in that in a mood, transient I trust, of mealy-mouthed decorum I preferred to use another term than imbibition for similar processes in the Central Sutherland complex; perhaps the confine of a Government publication sobered me. In the 1847 volume that we are examining, two contributions of Virlet's are of interest to us. A long footnote (pp. 297–8) in a short glacial paper is devoted to a summary of his views on the origin of metamorphic porphyry and imbibition-rocks. His second contribution (pp. 498–505) has the arresting title, *Observations sur le métamorphisme normal et la probabilité de la non-existence de véritables roches primitives à la surface du globe*. We may examine this more closely.

Virlet (1847*b*) begins by making approving references to certain aspects of the views of Fournet and of Scheerer. Both these masters were immensely interested, as were Durocher and many another at this time, in the sequence of crystallization in granite. Minerals of high fusion point appeared to crystallize after minerals of lower fusion point; Fournet explained this by evoking *sur-fusion*, a sort of undercooling, whilst Scheerer proposed that the granitic paste, 'une bouillie, épaisse et humide', before its solidification, was impregnated with water and so crystallized at lower temperatures

than a dry melt (p. 492). Virlet remarks that the water contained in sediments will obviously be available when they are made into gneiss and granite.

Virlet introduces the term *granitification* with the following statement:

'Le transmutation des roches sédimentaires en granite, ou leur *granitification*, pour employer un mot qui caractérise le phénomène, n'est donc pas plus difficile à admettre que leur changement en gneiss; il suffit seulement de supposer ou que l'action métamorphisante a été plus directe et plus intense, ou qu'elle a été plus prolongée' (pp. 499–500).

Granitification, decides Virlet, does not necessarily require a high temperature. This is indicated by the preservation of stratification structures in, for example, the Brittany granites, as can be demonstrated by inspection of the Paris pavements of blocks of these granites in which countless fragments of country-rock are enclosed (p. 501). This appeal to the Paris pavements is one that Virlet had made before and which was to be echoed by Delesse in 1858 and Michel-Lévy in his classic Flamanville paper of 1893–4 (p. 9). Virlet (loc. cit., p. 144) maintains that the best way of studying granites and what we should now call xenolithic rocks generally is by inspection of pavements, especially, as Michel-Lévy was to say later, *les jours de grande pluie*. Virlet gives this advice because 'une cassure brute ne le laisse apercevoir ni aussi nettement ni aussi distinctement que sur les dalles qui ont reçu un certain poli par suite du piétinement, surtour quand celles-ci sont mouillées. Il suffit donc, je crois, de parcourir les rues de Paris, après une forte pluie. . . .' I here allow myself to succumb to the temptation to another of my many digressions.

Intensely detailed studies, after the 'micropetrological school of Heidelberg', have of course their place in the investigation of granites, migmatites and so forth. But more important, to my mind, are studies of the largest possible clear rock-surfaces whether natural or artificial. Nowadays we do not have to wait for our large specimens to receive 'un certain poli par suite du piétinement'. There are provided, free to us, but at great cost to the brewers, bankers and other followers of Balbus, large and beautifully polished slabs of the most exciting stones. Every city has on free display magnificent examples of granitization and

migmatization. I suggest to members of this Association resident in cities that they should track down, record and describe the more outstanding of the polished slabs on public view. In London, for example, there are available great expanses pertinent to this address along Fleet Street, Victoria Street, Knightsbridge and Brompton Road—to name only a few of the better displays. The best samples known to me are provided by the pillars of St. Columba's Church, Pont Street, and by twelve columns disposed about the green in Parliament Square—these latter are known to my students, and rightly, as the Twelve Pillars of Wisdom.* During the bombing of London I watched anxiously for the detachment of specimens of more portable size from the finer exhibits, but without success; indeed, I formed the opinion that the safest procedure in a London blitz was to clasp one of the prize columns to your bosom—neither you nor it would be so much as chipped.

Durocher in his 'Note sur une espèce de granite provenant de la Normandie et de la Bretagne', in the 1847 volume, p. 140, criticizes Virlet's view that these granites are formed from sediments and contain pebbles of quartz; the alleged pebbles are quartz nodules, due, Durocher says, to accidents of crystallization. Virlet replies that no segregations occur in these granites, but that there are great quantities of fragments of sedimentary origin in them and maintains that some of the siliceous fragments retain their original marine-pebble shape. We need not discuss Virlet's last contention, but we may note the existence so long as a century ago of a sharp division of opinion on the origin of certain enclaves in granites—by some they are considered to be segregations, by others xenoliths. This discussion still persists; it is maintained by the fundamentally different views concerning the origin of granite advanced by the opposed schools. If we wish to keep the granitic magma as pure and undefiled as possible, then dark blots in the granite are thought to 'result from some obscure process of segregation or secretion' to quote the revealing words of Grout (1932, p. 61). If, on the other hand, we suppose that granitic magma must, by its very nature, act upon its walls or that granitic magma may be more correctly regarded as granitic migma, then the more blots there are the better—they will furnish the key to the rock's history.

* These exhibits are now, alas, no more (1956).

In the 1847 volume there is a translation by Frapoli of an important work by Scheerer, 'Discussion sur la nature plutonique du granite et des silicates cristallins qui s'y rallient' (p. 468). Scheerer inquires as to what are the facts which should serve as a basis for a theory of granite. Its plutonic origin has been thought to be shown by its contact phenomena and by the resemblance in its composition to that of some true volcanic rocks (p. 475). In Scheerer's opinion, neither of these are serious proofs of an igneous origin of granite and the problem can only be solved by a study of the intimate structure of the rock itself (p. 476). Such a study indicates that water must take a prominent part in the happenings in the granitic paste which must thus be a 'bouillie aqueuse' (p. 489). Scheerer favours imbibition as an important process of metamorphism at contacts (p. 494) and, in discussing the transformation of clay rocks into rocks having the appearance of gneiss and granite, concludes that such a transformation, as observed in Norway, is due to the heating-up of rocks containing water (pp. 494–5).

The greatest figure in geological circles in France at this time, and possibly at any time, was Élie de Beaumont. An excellent sketch of the man, Senator of France and *grand seigneur*, has recently been given by P. Fallot (1939, pp. 75–107), whilst an account of his work, including a summary of his 1847 essay on emanations which we examine in a moment, appeared in C. Ste-Claire Deville's volume *Coup d'œil historique sur la Géologie et sur les travaux d'Élie de Beaumont*, Paris, 1878. Author of some 250 geological works, Élie de Beaumont dealt in a masterly fashion with all branches of the science. We can here occupy ourselves with only one contribution, his *Note sur les émanations volcaniques et métallifères*, which occupied pp. 1247–333 of the 1847 volume of the Geological Society of France. As Fallot has remarked, this classic paper laid the foundations for the modern study of mineral deposits; I can deal here only with such parts of it as are relevant to my topic today.

The first point in Élie de Beaumont's work to which I direct attention is one of great interest to our present discussion. On several lines of evidence he concluded that there are two fundamentally opposed groups of eruptive rocks: first, the basic group which is dominantly volcanic, but includes also traps and

serpentines, and secondly, the acid group which is dominantly plutonic and granitic; each group is attended by its own cortege of characteristic mineralizers, mineral deposits and constituent minerals (pp. 1253, 1254, 1288). There are fundamental differences in the mode of eruption of granites and of other eruptive rocks (p. 1297). These statements of de Beaumont's are an early expression of the views on the volcanic and plutonic associations of eruptive rocks put forward by Kennedy and referred to in my address of last year.

The emanations with which de Beaumont is concerned are considered to be volatile substances capable of transporting silicate and metallic materials beyond the active zone of heat and, since they can clearly penetrate through the finest pores and spaces in rocks, they must be in the form of vapours or solutions. They take their origin, together with the lava which brings them to higher levels in the crust, in the internal furnace of the earth (pp. 1250, 1274, 1275, 1281, 1288).

The action of these emanations is especially well seen in and around granites with tin lodes attached to them. There is a community of elements characteristic of the granite, the tin lodes and the metamorphosed country-rocks. Thus arises an *aura granitica*, or a *pénombre du granite*, to recall Humboldt's phrase, in which chemical reaction and physical crystallization have proceeded with extraordinary energy; this leads to the exaggeration at the granite margin of the granitic characters so that there arise 'dégradations ou monstruosités de granite' such as pegmatite, graphic granite and greisen. The emanations must be in the form of a singularly active and tenuous vapour since they are able to transport very refractory substances and to produce great changes in the country-rocks (pp. 1295, 1296, 1314, 1315).

Granites are so often characterized by marginal passages to gneiss and mica-schist that the origins of all these crystalline rocks must be closely connected (p. 1301). De Beaumont accepts Virlet's proposals that some granites are of metamorphic origin and discusses how such granites can yet display the characteristic minerals of the *aura granitica*. The sedimentary rocks from which the mica-schists, gneisses and granites were formed by metamorphism had been derived, he suggests, from more ancient granites and gneisses, so that the characteristic minerals were

obtained at second-hand. This notion of cycles of these characteristic minerals or elements is one that in my opinion may yet play an important part in the elaboration of the granitization theory. De Beaumont repeats that the origin of mica-schists, gneisses and granites is one connected question; he says:

'Ainsi la question de l'origine du granite est à la fois d'autant plus importante et d'autant plus difficile, qu'elle comprend celle de l'origine de plusieurs des roches cristallines les plus répandues du globe. C'est une vaste problème dont la question de l'origine du granite est le nœud' (p. 1301).

This profound statement has been forgotten by many a thoroughgoing magmatist. For example, Niggli (1942, p. 7) believes that the association of granite and gneiss *complicates* the granite problem. For some of us, on the other hand, this association may *clarify* the problem. We will resume this debate later; I now return to Élie de Beaumont for a last point. He distinguishes very clearly between granites with chilled margins and but feeble contact effects and those surrounded by veins and stockworks. The first type evidently possessed *la vertue métamorphisatrice* to only a small degree and lost at their contacts the faculty of granitoid crystallization—'ils étaient, pour ainsi dire, *éventés*'. In their mode of emplacement and in their poverty in mineralizers, they resembled the quartz-porphyries and the basic group. This difference in the marginal relations of granites is a fundamental aspect of the granite problem and its recognition is by no means the least of Élie de Beaumont's contributions to petrogenesis.

The last of the masters of the 1847 volume that I have to consider is Fournet, whose early views I have already noted. In his paper, 'Résultats sommaires d'une exploration des Vosges', pp. 220–54, there are certain results of great importance to this discussion. Fournet, in agreement with Élie de Beaumont, is very clear that the metamorphism brought about by granites is due both to the heat that they have made available and to the 'principles' necessary for metamorphism that they have furnished to the country-rocks (p. 230). He distinguishes between eruptive porphyries and metamorphic porphyries, and in his discussion of the origin of the latter type mentions such ideas as the capillary soaking of metamorphic rocks by igneous liquids, the saturation of

country-rocks by the porphyritic paste, and the formation of porphyritic rocks by imbibition with concomitant loss of fissility by original sediments (p. 239). He realizes that if packets of country-rock float in an igneous bath their lower parts will be dissolved and according to their nature thus impress *une physionomie spéciale* on the plutonic rock. Comparable operations are commonplace in the chemical laboratory. A lump of sugar, half immersed in water, he recalls, draws up a certain amount of water into its upper parts and at the same time the excess water becomes charged with sugar. Are plutonic rocks to be placed outside the general rule?; 'seraient-elles des corps exceptionnels?' If they are exceptional bodies then

> '. . . tous nos raisonnements, tout le laborieux enfantement du métamorphisme par des effets de contact, de capillarité et de confusion, n'auraient abouti qu'à la production d'une vaine chimère à placer à côté des vapoureuses rêveries auxquelles se laisserait aller une science encore dans son enfance' (pp. 241–2).

Happily, says Fournet, this is not the case. Caution, however, is needed. Transitions of one rock into another allow us to make certain genetic conclusions, but the fundamental characters of the three great groups of rocks—igneous, sedimentary and metamorphic—remain unimpaired; if it were otherwise we should fall into the ancient errors and see passages from granite into coal, porphyry into sandstone, basalt into shelly limestone.

These statements of Fournet's lead me to two points. First, he saw very clearly that plutonic rocks were not emplaced as it were in platinum crucibles—there must be reaction at the walls. Secondly, he uttered a warning against excess of enthusiasm for rock transformations which should be taken to heart at this present time. Great damage is being done to the general hypothesis of granitization, for example, by the uncritical application of the proposal; we need, sometimes, saving from our friends.

After the general discussion which I have indicated above, Fournet deals in particular with the porphyries of the Vosges. He concludes that the metamorphosed rock has influenced the metamorphosing rock; there has been reciprocity of action. This leads him to one of his most important proposals. *Metamorphism*, he states, is the totality of effects in both the plutonic and country

rock at contacts; modifications of the plutonic rock are *endomorphism*, of the country-rock are *exomorphism* (p. 243). He thus supplied two terms which have remained peculiarly French as have, I am afraid, the ideas which underlie them. For Fournet and his contemporaries then, it was to be expected that plutonic rocks should be modified in structure and in composition by reaction with the country-rocks—they could thus acquire *une physionomie spéciale*.

In this same *annus mirabilis*, 1847, there appeared the German translation by W. Vogelgesang of a remarkable work of Fournet on metamorphism. The translation is entitled '*Die Metamorphose der Gesteine nachgewiesen in den westlichen Alpen*' (Freiberg, 1847) and is prefaced by an introduction by B. Cotta which is worthy of notice. Cotta admits that a sedimentary pile, if thick enough, must come within the influence of the earth's internal heat, and may be melted to give eruptive rocks which would produce contact metamorphism. Large granite masses must act in a similar fashion, though on a smaller scale, to the hot interior of the earth and so give rise to crystalline schists and even eruptive rocks. Cotta regrets, however, that he cannot remember finding rock transformations of this special kind on any great scale.

Fournet describes, amongst other phenomena, complex veining of country-rocks, transitional zones between country-rock and eruptive rock (op. cit., p. 6), felspathization, impregnation of country-rock (p. 12), and the growth of porphyritic felspars in hornstone (p. 13). His theoretical discussion contains abundant material of great interest to students of metamorphism. He attempts, for example, some kind of zoning in his metamorphic rocks; he endeavours to zone the metamorphism of clay-slate on colour, but cannot get far (p. 37); by considering the state of the various bituminous rocks of the Western Alps, he deduces a kind of zonal arrangement for them about a central plutonic axis, that is, an axis of access of the earth's internal heat—but finishes by pleading for more information (p. 36). Surely this must be one of the earliest explorations of metamorphic zoning.

Fournet discusses the results of plutonic action on slaty rocks. He notes that since clay-slate contains potash, soda and lime it is possible for felspars to arise by simple recrystallization. In some cases, however, so much felspar is developed that an impregnation

with felspar, by some kind of capillary action, must have taken place (op. cit., p. 72). He goes on to distinguish two kinds of process in the formation of these special contact rocks—a distinction which we shall see later has played an important part in the subsequent development of French ideas on granite contacts. Fournet distinguishes between *injection*, in which veinlets are inserted along foliation planes, and *capillary soaking* of hot fluids into the lamellae of the country-rock (pp. 73-4). The materials involved in the soaking permeate the country-rocks with extraordinary ease and must lead to important chemical reactions in them (p. 76).

Finally, Fournet shows that the degree of metamorphism is related to the proximity of a plutonic hearth (op. cit., p. 97). Metamorphism is, however, so widespread in many regions that it is rather difficult to relate it to eruptive bodies which have brought in the heat and materials necessary for metamorphism. According to some, it appears much simpler to permit a stream of hot gases to perform the rock transformations that are observed (p. 98).

So much for the year 1847. We have seen the planting then of a great variety of seeds. Some have parched and perished, others have germinated and have grown into the brightest flowers of French geology. Before I pluck a bunch of these I have to record the nurture of the tender plants by many a French geologist—I select Delesse for a brief inquiry.

In his *Études sur le Métamorphisme des Roches*, 1858, Delesse separated two great classes of metamorphic phenomena, those of normal or general metamorphism (our regional metamorphism) and those of abnormal or special metamorphism (our contact or thermal metamorphism). In contact metamorphism, dealt with in this 1858 volume, the responsible agents are heat and gases, vapours or solutions (p. 3), but the effects of heat have been exaggerated (p. 10). He notes that at granitic contacts, contact metamorphism confuses itself with regional metamorphism (p. 5). His treatment of such contacts follows of course the French mode. Among other matters of interest he describes, from the pavements of Paris, the development of granite minerals in fragments of country-rocks and recalls that Cotta had noted in 1857 the growth of large crystals of orthoclase in dioritic fragments in the Thuringerwald granite (p. 316). Similar felspathization is recorded in

the St. Gotthard area, in Norway, in the Vosges and elsewhere (pp. 362–4). In many cases, pelitic sediments pass by a process of felspathization into '*un véritable porphyre granitoide*'; when the felspar developed is orthoclase, it is usually accompanied by the minerals of granite—'dans ce cas, suivant l'expression consacrée par Keilhau, le métamorphisme est caractérisé par une *granitification*' (p. 364). This kind of metamorphism is usually on a grand scale and merges into regional metamorphism (p. 364).

In his 1869 *Études sur le Métamorphisme des Roches*, Delesse considers his general metamorphism or our regional metamorphism. During this consideration he advances ideas on the plutonic rocks which are of fundamental interest in this present discussion. His most important proposal is that the plutonic rocks are the result and not the cause of metamorphism. Thus:

'Les roches plutoniques se sont formées aux dépens des roches métamorphiques, et elles representent le maximum d'intensité ou le terme extréme du métamorphisme général' (op. cit., p. 88).

When plutonic rocks occur in veins they cause little metamorphism and that little is special metamorphism in Delesse's sense; when they occur in large masses, however, they pass insensibly into the neighbouring rocks and have great zones of metamorphism about them. Metamorphism with low energy gives rise to slates and similar rocks, whilst metamorphism with high energy gives rise to high-grade metamorphic rocks *and* plutonic rocks (pp. 88, 89). Delesse then makes the following observation—an observation linking earlier views such as those of Élie de Beaumont, with certain developments which we consider later:

'Ces dernières [i.e., the plutonic rocks arising through metamorphism at high energy levels] qui jouissaient d'une plasticité plus grande se sont très souvent comportées comme des roches éruptives. Elles ont été ramenées vers le surface du sol par des pressions intérieures. . . . Mais alors elles ne sont plus en contact aves les roches métamorphiques qui leur correspondent, ni avec celles aux dépens desquelles elles se sont formées; c'est, du reste, le cas le plus habituel' (op. cit., p. 89).

That is, cross-cutting granites at high levels in the crust may

have lost all evidence by which their real origin can be determined —a pleasing thought, indeed.

During the last quarter of the nineteenth century the French school blossomed in the work of Michel-Lévy, Lacroix, Barrois, Duparc, and others. When nowadays geologists refer to the French School of granitization, using this term in a broad sense, it is the products of this period that they have in mind but, great though its achievements then were, they were possible only because they were developments from the experience of the earlier parts of the century. Injection, imbibition, assimilation and the like had become part of the French geological mind. It was therefore in a congenial atmosphere that Michel-Lévy and Lacroix in particular produced those embellishments of the French corpus which we shall examine immediately. Before we do that, however, I should recall that these were the times when, outside France, the 'micropetrological school of Heidelberg'—to use Sederholm's term—headed by its world-famous leader Rosenbusch and supported by industrious cohorts from many European and American satellites marched in its greatest array. The temporary conquests of this school were innumerable and extensive. We should remember in particular at this time* that the French citadel of 'granitization' maintained itself inviolate against all assaults, largely because of the brilliant sallies of Michel-Lévy and Lacroix. We have opportunity here to examine in detail only two campaigns, first that of Michel-Lévy, of the line of Élie de Beaumont at the Collège de France, and then that of Lacroix of the Muséum.

I select for special remark only one of the many great contributions of Auguste Michel-Lévy—the Flamanville classic (Michel-Lévy, 1893–4). At Flamanville in the Cotentin, on the western shore of the Cherbourg peninsula, Palaeozoic sediments are invaded by a small mass of granite which supplies well-displayed contact phenomena.

Michel-Lévy, following his forerunners in the French School, records the two types of contact process, *par injection et par superposition*, and defines them in the following terms:

'Par injection parce que les petits fillonets feldspathiques injectent lit par lit la roche schisteux préalablement transformée en schiste micacé; par superposition lorsque les éléments du granite de plus

* March 1944.

grosse taille, font pour ainsi dire éclater les éléments du schiste micacé et en englobent les débris sous forme d'inclusions' (op. cit., p. 4).

At the contacts the Silurian sediments are transformed into felspar-bearing rocks and pass into gneisses and micro-granites (op. cit., p. 6), whilst the granite itself becomes endomorphosed with the development of a schistose texture and the appearance of ferromagnesian minerals in quantity (p. 9).

Armed with his experience of other granite contacts and refreshed by these Flamanville displays, Michel-Lévy proceeds to discuss granites in general. As a preliminary, he declares (op. cit., p. 17) that structure (i.e., texture) and mineral composition are the only factors necessary for rigorous definition—a declaration worth pondering. What happens at granite contacts next occupies his attention. The dominant agents here are the mineralizers which act differentially on the various country-rocks (pp. 18, 21). At the immediate contact the effects of transfer of material are visible to the naked eye; there then arises a zone of felspathization in which gneisses of various kinds are found (p. 21). From the French experience it is certain that the views of Rosenbusch that felspathization does not occur at granite-contacts and that gneisses are not formed in such contacts are demonstrably wrong (pp. 21–2).

This felspathized zone has great theoretical significance; in it are found, synthetically reproduced as it were, all sorts and conditions of crystalline schists and in many cases these varied crystallines can be related to varying beds of country-rock. Michel-Lévy records that Barrois had provided in the then not yet published accounts of the Brest and Morlaix Sheets irrefutable examples of the transformation of entire regions of Pre-Cambrian beds into gneisses by granitization. Bands of quartzite, more resistant to felspathization, can be traced in the midst of more argillaceous rocks which have been converted into gneisses. 'La stratigraphie vient donc ici affirmer l'équivalence de vrais gneiss avec des schists franchement clastiques, dont la transformation est due au voisinage de massifs granitiques' (op. cit., p. 23).

The study of enclaves in granites reinforces these conclusions. But a new and most significant fact emerges from this study. Great crystals of orthoclase, '*dents de cheval*', are found in the enclaves

identical with those which at times give a porphyritic appearance to the surrounding granite. Sometimes these crystals are scattered through the enclave, sometimes they orient themselves in accord with the schistosity, which has already been designed by the disposition of the granulitic and biotitic elements. Such a *gneiss glandaleux* cannot be formed, as Rosenbusch vainly thinks, by dynamo-metamorphism since the granite itself is not laminated (Michel-Lévy, op. cit., p. 23).

This skirmish with Rosenbusch is resumed later (op. cit., pp. 28–9) in connexion with the interpretation of marginal schistose varieties of granite found at many contacts. These were considered by Rosenbusch and his followers to be the result of dynamo-metamorphism; Michel-Lévy shows that they are due to impregnation of originally schistose rocks by granitic elements—the schistosity in the granite is relict from the sediment. Michel-Lévy demonstrates this by three lines of evidence, first the similarity between such schistose granites and certain enclaves, second the marked chemical changes which are inexplicable by dynamo-metamorphism and lastly the great differences which exist between these schistose marginal granites and those, as for example, from Mont Blanc, on which dynamic metamorphism has admittedly operated. I detail these arguments because even now the Rosenbusch position is maintained in some strength. A point demonstrated by Michel-Lévy by comparative studies of granites in the Morvan, Beaujolais, Auvergne and Mont Blanc is that the feldspathized zone around granites increases with depth until the contact zones in some measure become zones in which granitic rocks are made (op. cit., pp. 22, 24).

We have already noted that Élie de Beaumont and others of his time clearly recognized a fundamental distinction between the basic and volcanic rocks on the one hand and the acid and plutonic on the other—early expression of the idea of Volcanic and Plutonic Associations. Michel-Lévy stresses this distinction. In his major polemic against Rosenbusch, his *Structures et Classification des Roches Eruptives*, 1889, p. 84, he had rejected the view of any direct relationship between the granitic rocks of the depths and the volcanic rocks of the surface. The volcanic chimneys had nothing to do with the *appareils granitiques* (Michel-Lévy, 1893–4, p. 34). He underlines this conclusion:

'Nous ne saurions donc trop insister sur l'absence de liaison stratigraphique ou pétrographique entre les laccoliths, les vraies cheminées volcaniques avec filons et lits intrusives, d'une part et les gisements granitiques d'autre part ' (op cit., p. 35).

Michel-Lévy's last topic is concerned with the eruption and *mise en place* of granites. Suess' concept that granite batholiths result by the simple filling of spaces arising through tangential movements of the crust is clearly wrong, since it is a fact that granites enlarge themselves downwards and 'gagnent aux dépens des formations cristallophylliennes voisines qui deviennent, elles aussi, de plus en plus cristallines et granitiques' (op. cit., p. 36). Study of favourable exposures, such as that of the great quarry of Diélette at Flamanville, shows that granites make their way very slowly without disturbance of the invaded strata. 'Tout milite en faveur d'une tombée en fusion, en liquation, des salbandes voisines, avec assimilation lente et partielle par la roche éruptive' (p. 36). Marginal inhomogeneity arises by the incomplete digestion of a crowd of fragments of country-rocks. Granites are locally charged with very local strata. For example, at several French localities, the granites show nodules of cordierite, still in regional alignment; these nodules are the only relics of the remainder of the country-rocks which have been resorbed in place (p. 36).

It seems, says Michel-Lévy, that the ascent of granitic magma takes place by a kind of fusion, assimilation or corrosion of the surrounding rocks, brought about by heat and mineralizers coming from the magma. First stages are seen in the rejuvenation of quartz and the formation of black mica in the contact-rocks. Then felspathization takes place, little veins of quartz and felspar are born which increase in number till they are inseparable from the granite and have made mobile all the elements of the contact-schists. Michel-Lévy concludes:

'En d'autres termes, le métamorphisme de contact se confond peu à peu en profondeur avec le métamorphisme général. Ils unissent leur action et si l'on supposait l'érosion suffisamment profonde, on arriverait au niveau où les magmas granitiques ont dû fondre tous les voussoirs de l'écorce terrestre' (op. cit., p. 36).

'Il en résulte que les voussoirs affaissés, qui auraient pu leur servir

de base, sont plus ou moins assimiliés par la roche éruptive et transformés en gneiss granitiques, puis en granites gneissiques et enfin en granite' (op. cit., p. 39).

Michel-Lévy, by his work in the field and in the laboratory, gave precision to the proposals of his predecessors of pre-microscopic petrology days. Imbibition, injection, lit-par-lit action, felspathization, endomorphism, inherited textures, depth-phenomena were elaborated. His greatest contribution, however, was his views on the mode of emplacement of granites by the gradual making-over of the wall rocks until they come to resemble granite and the incorporation of this material in the granitic magma. It is difficult to assess how much of Michel-Lévy's process would come within my definition of granitization. To him granitic magma is always in evidence at the higher levels, whilst in depth granitic magmas may be formed by the melting of the walls. It is clear, however, that much of his felspathized zone and of his endomorphic granites would comply with my requirement that the magmatic stage has not been passed through.

Of the relevant work of A. Lacroix, I take for consideration the two famous memoirs on the granite contacts in the Pyrenees—'Le Granite des Pyrénées et ses Phénomènes de Contact: Premier Mémoire', *Bull. Ser. Carte Géol. France*, No. 64, vol. x, 1898–9; 'Deuxième Mémoire', *ibid.*, No. 71, vol. xi, 1899–1900. From this study of well-exposed contacts of granite with very varied country-rocks in the High Pyrenees, Lacroix shows the general applicability of Michel-Lévy's conclusions on the contact-phenomena and the mode of emplacement of granites. Lacroix (1898–9, p. 48) contrasts the views of the French, as put forward by Michel-Lévy and himself, with those of the Germans, as put forward by Rosenbusch. It is worth reproducing here, in Lacroix's own words, what the two positions were:

According to the French (Lacroix, 1898–9, p. 48):

'Les phénomènes de contact des roches éruptives sont le résultat de la transformation d'une roche préexistante, apportant sa caractéristique personelle, sous l'influence d'agents minéralisateurs, le plus généralement accompagnés d'éléments volatils ou solubles qui, en se fixant la roche modifié, en transforment plus ou moins complètement la composition chimique.'

According to the Germans (Rosenbusch, 1896, vol. ii, p. 85):

'On peut formuler cette loi, que dans le métamorphisme de contact au voisinage des roches de profondeur, la roche éruptive a agi d'une façon purement physique et en général n'a agi chimiquement par l'intervention d'aucune substance. L'analyse chimique des roches de contact dans les divers stades de leur évolution a montré, qu'à l'exception des corps volatils comme l'eau et les substances organiques, il n'y a pas eu changement dans la composition. Tout s'est borné en réalité à une nouvelle répartition des molécules; la roche éruptive par les conditions de température et de pression dont elle était le foyer en est la cause, alors que la roche sédimentaire en a formi la matière.'

Lacroix makes appeal to the only evidence of value, that displayed in the field. He recalls that at many contacts in the Pyrenees described by himself and at many other places in France described by Michel-Lévy and Barrois there is a constant zone of felspathization by imbibition and by injection at the immediate contact of the granite; in this zone it is possible to follow progressively all the stages in felspathization and to trace insensible passages between the felspathized schists (leptynolites) and the granites themselves; but even in the most felspathized parts, where true gneisses are formed, there are still recognizable relics of the country-rocks in an intermediate stage of transformation (Lacroix, 1898–9, p. 49). Such phenomena are so often seen at French contacts that Lacroix is astonished at the existence of other views. But, he says, 'je n'aurai gardé de confondre la *possibilité* pour le magma granitique de déterminer des felspathizations intenses, avec la *nécessité* pour celui-ci d'agir toujours de la même façon sur les roches sédimentaires en contact avec lui' (p. 50). Admittedly, some granites show knife-edge contacts with hornfels zones about them; this variation in type is clearly due to variation in the amount of mineralizers, in the physical conditions at the contacts, and, especially, in the depth. Transfer of material is obvious (p. 51). There must, for example, be transfer in the formation of metamorphosed limestones rich in potash felspars (p. 52).

Lacroix agrees with Michel-Lévy (1897, p. 326) in considering that magmas consist of two parts, one which consolidates as the ordinary rock-forming minerals, the other, set free when this

consolidation occurs, composed of volatile elements which are able to modify profoundly the country-rocks adjacent to deep-seated magmas. It is clear in the Pyrenees granite contacts that the elements contributed to the country-rock when added to the substances already there produce a rock having a composition very near that of granite itself; the solution of this modified material by the granite magma does not alter the composition of the magma. Granite magma is thus the result of the assimilation of schists and is not the original pure magma (Lacroix, 1898–9, pp. 56, 57). 'Le magma, ayant dissous les schistes, devait avoir la composition du granite consolidé sur lequel sont basées nos observations, augmentée de la partie volatile libérée au moment de la consolidation de la roche' (p. 57). If much pelitic rock is dissolved, the granite is over-rich in biotite, if calcareous rocks are dissolved the granite is more basic.

The part of Lacroix's Pyrenean work which has given rise to most discussion is that dealing with the endomorphism of granite by limestone. Isolated in the granite, since the non-calcareous rocks have been dissolved, are lenticles of limestone, and closely associated with these are bodies of hornblende-granite, diorite, olivine-norite, hornblendite and hornblende-peridotite—that is, bodies of a variety of plutonic igneous rocks (op.cit., p. 59). Michel-Lévy 1896, p. 123; 1897, p. 369) had noted a somewhat similar association in the Beaujolais and at Puy-de-Dôme, and had proposed that endomorphism by calcareous rocks had taken place. This process, combined with felspathization, was considered by Michel-Lévy to play a fundamental role in the evolution of the magmas which give the eruptive rocks; Michel-Lévy's proposal was, of course, entirely opposed to 'les diverses théories de différentiation *en vase clos* fort en honneur aujourd'hui parmi les pétrographes'; *en vase clos* is what I have before termed *in platinum crucible*.

Lacroix (1898–9, p. 59) is careful to point out that he does not wish to claim that all this variety of plutonic rocks is everywhere due to the assimilation of limestone by granitic magma, but he rightly insists that his field-observations show that the required transformations are *possible*. Every time, he reiterates (p. 60), that one finds a lens of limestone enclosed in the granite, one sees it surrounded by a peripheral zone of rock rich in amphibole, and

this amphibole-rich rock is found only around the limestone or on the strike of the limestone outcrops.

Lacroix agrees further with Michel-Lévy's views on the *mise en place* of granites by the progressive assimilation of the country-rocks whose place they come to occupy. The retention by enclaves of their original positions in the regional structure is convincing proof of this thesis (Lacroix, 1898–9, p. 62). 'Dans la vallée de Baxouillade, il est facile de suivre pas à pas l'imprégnation et la gneissification des schistes formant encore par places des traînées continues au milieu d'une roche, qui oscille entre un granite gneissique et un gneiss granitoide à enclaves' (p. 63). In these Pyrenean contacts there has thus been no kneading of the complex and the granite has reached its upward limit, the result being that the endomorphosed products still lie beside the enclaves responsible (p. 63).

Lacroix emphasizes the high validity of the field evidence—*nous pouvons toucher de la main*, he says, and adds that these matters are not *une simple conception de l'ésprit* (1899–1900, p. 26). The gradual ascent of the granitic magma is described as follows:

'Au fur et à mesure que le magma granitique montait dans les couches sédimentaires, il y était précédé et accompagné par son cortège d'émanations transformatrices et, ce qu'il devait dissoudre pour faire sa place, ce n'était plus les sédiments normaux, mais des roches imbibées, transformées ou en voie de transformation, par fixation des émanations parties de sa propre masse' (op cit., p. 26).

Lacroix emphasizes the dominance of imbibition over injection in felspathized and granitized zones (op. cit., p. 27). He emphasizes once again the fundamental difference in the behaviour of pelitic and calcareous country-rocks towards the granitic magma; the former are felspathized until they attain a granitic composition (p. 27) and so, on absorption, cause little change in the magma (p. 29), whereas the limestones, being converted into calc-silicates and not into felspathized rocks, naturally modify the granitic magma into others solidifying as much more basic plutonic rocks (pp. 29–30).

The impatient reader, not appreciating the meandering and leisurely progress of meditations, mine or anyone else's, may protest that I have devoted a disproportionate amount of time to

the French school and especially to Michel-Lévy and Lacroix. I reply that I have deliberately considered in some detail the evolution of this school and its fruition in the eighteen eighties and nineties, because the British and American outlook on these matters has up to recently been largely Teutonic. Even some of us who render lip-service to the French school have but a slender acquaintance with its tenets. I have still more to say of this school in the work of Pierre Termier.

In considering the views on metamorphism and plutonic activity put forward by Termier we have to note with some care certain differences between him and his predecessors. To begin with, Termier (1904, p. 583–5) held that regional metamorphism is quite another matter than the intensified contact metamorphism of Michel-Lévy and Lacroix. The intrusion of eruptive rocks is only an episode in regional metamorphism—intrusion and metamorphism are indeed but two effects of the same cause. 'S'il y a des gneiss dans le Mont-Blanc, et des micaschistes, ce n'est parce que le granite est venu s'installer au milieu de ces strates: mais le granite est venu se former au milieu de ces strates sous l'empire de la même cause qui, de ces strates sédimentaires, faisait des assises cristallines.' Regional metamorphism is produced by an oil-spot mechanism, the celebrated *tâche d'huile* mechanism, by which, just as oil spreads unequally through the layers of a pile of different stuffs, so do elements arriving from the depths permeate unequally into the sedimentary layers in the geosynclinal pile (loc. cit., p. 585). Influx of material is clearly operative: 'cet apport, je le demande à des colonnes filtrantes, venues d'en bas, et qui montent, comme d'une chaudière, du fond de la région centrale du géosynclinale' (loc. cit., p. 585). The appearance of felspars is undeniable evidence of transfer of material (Termier, 1912, p. 590). But for the whole series of crystalline schists, Termier affirms, one cannot reasonably doubt that there has been a new influx of material from the depth which has chased in front of it some of the old material (loc. cit., p. 590): this is the 'migmatite front' of modern writers. These juvenile vapours, *véritables colonnes filtrantes*, with various gases, silicates and borates of the alkalis, in passing upwards raise the temperature of the surrounding rocks; this rise, with the abundance of solvents, leads to intense chemical reaction; mixtures with low melting points go into solution first,

whilst the old elements in excess flee before the *colonne filtrante* and displace other elements in their turn. True magmas are thus formed here and there of all dimensions and increasing in volume with depth. When the whole region cools down, the parts which have become fluid crystallize as massive rocks, granite, gabbro, diorite and so on—each great family of igneous rocks corresponding to a eutectic. In the upper parts of the structure, crystalline schists are formed, whilst the still liquid magmatic portion may be intruded at high levels in cross-cutting fashion (loc. cit., pp. 592–3).

For Termier, then, the production of igneous rocks is only an episode in regional metamorphism. The sole criterion of abyssal formation is the amplitude and intensity of the metamorphism which surrounds the igneous rocks and which at first sight *seems* to emanate from them. If a granite is surrounded by a narrow aureole, it is certain that the granite has not been made *in situ*—it has come from somewhere else, ready made. If, on the other hand, a granite is surrounded by a vast aureole of metamorphic rocks, it has certainly been formed in place, by complete fusion of a eutectic, whilst the neighbouring rocks were regionally metamorphosed.

The chief difference (and this only for certain levels) between Termier, on the one hand, and Michel-Lévy and Lacroix on the other, is concerned with the status of the granite magma. For Termier it is generated by an intensification of the processes giving the crystalline schists and gneisses; for Michel-Lévy and Lacroix the granite arrives from the depths and is responsible for the extended metamorphism around it. It seems to me that the views as to what happens deep in the structure become confluent, as do in fact the fields of contact and regional metamorphism.

Modern French geologists, for example, Demay, Raguin, Longchambon, Jung, Roques and others, whilst in some respects elaborating the classic French views, have much in common with the Fennoskandian workers whose results I examine immediately. Regretfully, then, I can do here no more than recommend their writings to your consideration.

As I see it, the great contributions of the French during the last century have been these. First, they have made the geologist (if he does not keep his mind *en vase clos*) familiar with the notion of emanations, mineralizers, volatiles and so forth as agents of immense importance in metamorphic and plutonic phenomena.

Secondly, they have dissected and interpreted granite-contacts in such perfection that their century-old notions, and even their terminology, are still current. Thirdly, they have refused to believe that magma does not act upon its wall-rocks and have demonstrated reciprocal exomorphic and endomorphic operations of great significance. Fourthly, they have shown the existence of many kinds of granite-contacts, some sharp and some hazy, some with no transfer and some with immense transfer, and have thus displayed the unity of granite phenomena. In doing this, they have freed us from the illogical constraint of the gross extrapolations of Rosenbusch and his followers. Compare the beauty and fruitfulness of the French concept of the relations between depth and character of granite contacts with the barren frigidity of the Barr-Andlau master. Fifthly, they have considered the association of metamorphic rocks, gneisses, mica-schists, &c., with granites as of genetic significance and not as unnecessary complications. Sixthly, they have realized the significance and importance of felspathization as a petrogenetic process. Seventhly, they have advanced a mechanism for the emplacement of granites which solves the otherwise insoluble room-problem of granite intrusion. Lastly, they early appreciated the fundamental difference between the dominantly basic and volcanic association on the one hand and the dominantly acid and plutonic association on the other. This is a record which should make any man, French or not, proud to follow even at a distance.

IV. THE FENNOSKANDIAN CONTRIBUTIONS

In this section of my remarks I propose to place before you certain contributions by those masters of the migmatites, the geologists of Finland, Sweden and Norway. As I wish to deal more particularly with recent results from these countries, I have no opportunity to consider the views advanced for example by Keilhau and Scheerer over a century and by Kjerulf and Reusch over half a century ago. I begin with Sederholm.

In the first part of these Meditations on Granite (Read, 1943; this volume, pp. 65-7) where I discussed the granitic magma and its origin, I considered various aspects, concerning migmatites,

ichors, anatexis, palingenesis and so forth, of the researches of the great Finnish geologist, J. J. Sederholm (1863-1934). Sederholm's chief work is contained in four memoirs of the Commission Géologique de Finlande, and interim reports, summaries and discussions of his are numerous in various non-Finnish publications. The four memoirs we are concerned with are, first, the classic 'Om Granit och Gneiss', *Bull. Comm. Géol. Finlande*, No. 23, 1907, written in Swedish but with an English summary of twenty pages, and second, the three volumes in English 'On Migmatites and Associated Pre-Cambrian Rocks of South-western Finland', No. 58 (1923), No. 77 (1926) and No. 107 (1934). This last memoir, No. 107, was edited and published after Sederholm's death by Erkki Mikkola, who himself perished in the first Finnish-Russian war. These four memoirs are illustrated by hundreds of figures of rock-surfaces which display the astounding complexity of the Finnish Pre-Cambrian, so that merely to turn over their pages provides a liberal education on the possibilities of migmatization. Here, however, I have opportunity to deal only with such parts of Sederholm's work as are necessary for the present inquiry. I leave his dissection of the Finnish granitic complexes for your study.

As appears to be also the case with Michel-Lévy and Lacroix, nowhere, as far as my knowledge goes, does Sederholm explicitly define granitization, though he uses the term dozens of times. He refers for example to Michel-Lévy's *granitization theory* (Sederholm, 1923, p. 5) and proclaims himself to be 'one of the most eager partisans of the granitization theory' (ibid., p. 13). We have therefore to endeavour to extract from his writings what he understood by granitization; I believe this can be done reasonably and fairly. In practically all cases, granitization was to Sederholm a process clearly connected with granitic magma and was caused by it: thus, 'phenomena of granitization caused by not less than five different granites' (ibid., p. 17); 'granitization phenomena of Hangö granite' (ibid., p. 100); 'it is sometimes difficult to say whether the granitization is due to Vätskär granite or to the Rysskär granite' (ibid., p. 127); 'granitization at the time of Hangö granite' (Sederholm, 1926, pp. 34, 69); 'granitization processes which are connected with the eruption of these different granites' (ibid., p. 36).

Inquiry as to what this granitization was, shows, I think, that it comprised all processes of mixing which gave rise to the rocks that Sederholm styled migmatites. For example, he describes 'a number of typical instances of granitization caused by these granites, beginning with those in which the two constituents are better separated and ending with the more completely mixed rocks' (Sederholm, 1923, pp. 101–2); the typical instances given include veining, lit-par-lit striping, solution during veining, development of patches of granite in the country-rocks, the formation of eruptive breccias or agmatites, and the endomorphism of the granitic magma itself (ibid., pp. 102–10). Again (ibid., p. 128) the formation of eruptive breccias is called a 'stage of granitization'; further (Sederholm, 1926, p. 55) an example of granitization is provided by an injection 'by aplitic veins which very intimately permeated the older rocks, so as *to change . . . them to migmatites*' (my italics). It appears to me to be clear that granitization and migmatization were more or less equivalent to Sederholm. We may examine migmatization a little closer.

In his preliminary memoir, 'Om Granit och Gneis', 1907, p. 110, Sederholm had defined migmatite as follows:

'For the gneisses here in question, characteristic of which are two elements of different genetic value, one, a schistose sediment or foliated eruptive, the other, either formed by the resolution of material like the first or by injection from without, the author proposes the name of *migmatites:* the position of this rock group being intermediate between eruptive rocks, proper, and crystalline schists of sedimentary or of eruptive origin.'

Twenty years later Sederholm again defined migmatite:

'The present writer thinks it necessary to use a designation for these hybrid rocks which really characterizes their appearance and origin. They look like mixed rocks, and they originated by the mixture of older rocks and a later erupted granitic magma, and therefore the name *migmatite* is the most appropriate' (Sederholm, 1926, p. 136).

The eruptive components of migmatites 'may belong to different granites, sometimes to two or even more of them' (ibid., p. 136). 'With the more complete assimilation of the older rocks, it will in some cases be rather difficult to determine the original character

of the older components, e.g., to discriminate between granitized gneissose granites and very strongly granitized leptites' (ibid., p. 136). Migmatites are, to Sederholm, always connected with granites.

I select certain aspects of these remarks of Sederholm's for discussion. The first concerns his requirements that migmatites should 'look like mixed rocks'. This is a matter that I touched upon in a preliminary fashion in my address of last year where I recalled that Niggli also had included this clause in his definition of migmatite (P. Niggli, 1942, p. 36). Likewise for K. H. Scheumann (1937), who has dealt with the nomenclature of migmatites, a coarsely mixed structure is necessary in these rocks; this structure arises by the heterogeneous mixture of older solid rocks and younger melt solutions of different origins. This insistence on a coarsely mixed structure appears to me to give rise to considerable difficulties in any rigid application. It will be recalled that the products of imbibition, permeation, superposition and similar processes were familiar to the French in many granite-contacts; such products were in most cases by no means coarsely mixed. Further, Sederholm has described similar operations and similar products in the Finnish complexes. For example, in the 1907 'Om Granit och Gneis', we find figured and described a migmatite in which 'the whole rock is penetrated by a network of veins, in which the material is more massive than in the other parts, although not sharply defined from them' (op. cit., p. 97; Fig. 1, Plate IV). That the country-rocks are permeable is shown by the granitization of quartzite and the contact phenomena around the Rapakivi granites, for example (e.g., Sederholm, 1913, pp. 176–7; 1923, p. 88). Thus the magma or its juice is able to penetrate and permeate the country-rocks (Sederholm, 1923, p. 91, 1934, pp. 36, 41, 44, 52). The Hangö granite penetrates the country-rock 'intimately so that no boundaries are visible' and has 'soaked through the older rock masses, penetrating them *à tâche d'huile*' (Sederholm, 1926, p. 69). If 'no boundaries are visible' it appears to me to be unlikely that 'coarse mixing' will be discernible. I have seen in Finland masses of homogeneous rock which Sederholm himself demonstrated as migmatites. Anyone who has worked in migmatite areas will admit that the products of imbibition soaking or permeation are usually medium-grained

and homogeneous. Are these undoubted results of mixing to be excluded from the migmatites, the mixed rocks? It seems to me that no useful purpose can be served by retaining this restriction. It is, after all, a question of scale and anyone who takes too much thought of scales should re-read his *Gulliver's Travels*. Let us then agree that migmatites need not be *coarsely* mixed rocks.

A second aspect of Sederholm's definition, and one also on which I touched last year, concerns the necessity of a granitic magma being one parent (or perhaps a grandparent) in migmatization: 'even if a part of the veins may be derived from older material which has been dissolved, this dissolution has . . . taken place in connexion with the protrusion of a granitic magma' (Sederholm, 1926, p. 136). This is, as we have seen, the position of Michel-Lévy and Lacroix. A new magma which might become available for migmatization through this 'dissolution' is the palingenetic or newborn magma of Sederholm and the process by which such palingenetic magma is made from solid rocks is anatexis. Before I carry this discussion further, I must state that I have never felt the necessity for *both* of Sederholm's terms, palingenesis and anatexis. In many cases, Sederholm used them as interchangeable and I suggest that the term palingenesis should be dropped, since anatexis is able to do all its duties. We may now look into this process of anatexis; we shall find some unexpected nebulosities.

In his preliminary investigation of the Finnish migmatites as given in his 1907 'Om Granit och Gneis', Sederholm suggests that in the coast region of South Finland 'the phenomena of refusion or re-solution have occurred so extensively that it must be assumed that the whole area has been in a melting condition' (op. cit., p. 101). This substantial fusion, or re-solution, is brought about by the action of magma which, set in motion by processes resulting from the earth's cooling, eats its way to higher levels in the crust. 'With increased height, the dissolving and metamorphising power of the magma gradually diminishes' (p. 102). Sederholm continues: 'This process of regional resolution . . . or *anatexis* . . . of the solid rocks, does not materially alter the composition of the magma, taken as a whole, because it must assimilate almost *all* the material resulting from the destruction of the magmatic rocks, whose original composition is thus restored' (p. 102). Following Hutton, Sederholm sees:

'The process of the destruction and gradual renewal of the solid part of the earth's crust as a *circulation*. The granitic magma, once solidified and, in part, decomposed, undergoes again, when brought into the deeper parts of the earth, a resurrection, or . . . *palingenesis*. The regional metamorphism is caused by a weaker form of the plutonic forces, which at greater depth manifest themselves as regional re-solution or anatexis' (op. cit., p. 102).

Admittedly, I am abstracting from the short English summary of a long Swedish paper—and that a preliminary paper—but, even so, I find it very difficult to decide how far a magma is concerned in anatexis and what is its status if one is concerned. But let us pass on to consider a further statement of Sederholm's which appeared in 1913 (Sederholm, 1913, p. 175). In this general paper on migmatites, he remarks that anatexis is due to higher processes than those of metamorphism; anatexis is, in fact, ultra-metamorphic; it includes all those processes by which migmatites are formed, together with those characterized by an amalgamation or quickening between rock already solid and a later intruded magma. This statement suggests to me that anatexis and migmatization were in 1913 not very different matters. The same idea arises from the consideration of Sederholm's account of an eruptive breccia formed by blocks of basic rock in a cement of granite which is given in his first memoir on migmatization ten years later (1923, p. 125). After describing the results of the intimate penetration of granitic magma into the basic fragments, Sederholm says: 'When the anatexis has gone further, the nebulitic migmatite often looks like a granite or granodiorite containing Schlieren of a more basic material' (op. cit., p. 125). Later he remarks: 'When anatexis has proceeded far, the granitized basic rock has often acquired a granitic texture even if it was originally a metabasalt' (op. cit., p. 126). It appears to me from Sederholm's own description of these processes that no production of new magma by refusion need have occurred. Whatever uncertainties we may detect in Sederholm's usage of it, we must admit that there is need for this term anatexis provided it is properly and simply defined. For my part, as I said last year, I take *anatexis to mean the process of forming new magma from solid rocks*. We examined proposals concerning this process last year and we can continue that examination now with special reference to the Fennoskandians.

We may see a not unexpected evolution in Sederholm's ideas during his life-study of the migmatites. At times he was impressed by the great depths in the crust to which (in his opinion) the Finnish Pre-Cambrian rocks had been depressed. The regional refusion in South Finland resulted when 'the whole area . . . was once sunk to so great a depth, that it approached the bottomless magmaocean or tectosphere of the earth' (Sederholm, 1907, p. 101). In his Huttonian circulation, as we have noted, he envisages a resurrection of magma when denudation products are brought 'into the deeper parts of the earth' (op. cit., p. 102). The mountain-making processes, the metamorphism and the protrusion of the great grantic masses are only different phases of the same plutonic activity (op. cit., p. 109). By 1926, however, he emphasized the importance in anatexis and migmatization of metasomatism or ultrametasomatism brought about by granitic juices—his *ichors*. For example, concerning the migmatites of the Barosundsfjard area, the following statements which I recommend for your careful consideration are made:

'. . . These bedded rocks have been . . . intimately mixed up with granite, or granitic juices, transforming them metasomatically into granite-like rock' (1926, p. 25).

'In the migmatites of Bagaskär and Lövgrund we were able to study the change of the lighter portions of the bedded leptites, by metasomatic changes caused by the eruption of the oldest granite, into rocks of a more and more granitic character . . . a process which may be described as palingenesis' (op. cit., .p 27).

'There is in these rocks no marked limit between what could be called metasomatic ultra-metamorphism, and the injection of granitic magma' (op. cit., p. 28).

'. . . a leptitic schist has been ultrametasomatically . . . changed to such a degree as to reach the stage of refusion' (op. cit., p. 29).

'. . . the granitic juices penetrate certain layers and gradually change them metasomatically into rocks with a more or less typical aplitic composition' (op. cit., p. 29).

'The "juices" of the younger granite permeated the older one . . . and later it changes them "ultrametasomatically" into a new granite which in the final stages was able to react on other rocks like an eruptive' (op. cit., p. 50).

Sederholm goes on to state that his opinions 'have developed in

such a direction that he is inclined to ascribe more and more importance to the granitic exudations or "granitic juices", or what have been designated by French geologists as *les minéralisateurs*, an expression which has, however, not exactly the same meaning' (op. cit., p. 87). Sederholm is not attracted by this word juice (as having a vegetable flavour about it) and, after careful consideration, decides to use the word ichor instead. Ichor is derived from a Greek word meaning blood of the gods, but also serum or pus, and personally, I prefer using the sweeter juice rather than Sederholm's possibly pathological ichor. The granitic ichors, then, are exudations—'things showing gradations between an aqueous solution and a very diluted magma, eventually also a magma containing much water in a gaseous state' (op. cit., p. 89). They do not cover the gaseous matter involved in typical pneumatolysis (op. cit., p. 89).

Sederholm agrees with the French that great quantities of solvents are necessary for metamorphism; 'these solvents are in general exudations from eruptive masses, either visible or hidden at great depth' (op. cit., p. 129). Ichors are active also in auto-metamorphism and in the promotion of deuteric changes; 'occasionally, even a complete recrystallization of an already solid granite may take place when it is soaked in its own ichor' (op. cit., pp. 133–4). Soaking of the country-rocks by granitic magma and especially by granitic ichor is reaffirmed by Sederholm; 'the granitic magma often advanced *à tâche d'huile*' (op. cit., p. 134). This soaking may lead to the formation of new magma; 'the ichor of the granite has been able intimately to penetrate the older rocks giving them a new, "palingene", eruptivity, even in cases where their chemical composition has remained unchanged. In most cases, however, they have been gradually changed, when soaked with the granitic magma, or its ichor, and their composition has more and more approached that of the new granite' (op. cit., pp. 134–5). This intimate penetration by the ichors, combined with the mechanical injection of the older rocks, gives rise to the immense variety of the migmatites. Though these processes might be described as ultra-metamorphic, since they clearly surpass in strength ordinary metamorphic processes, they are really essentially different from these because they lead to 'the formation of rocks which are no more crystalline schists, but in which at least

one of the components behaves as an eruptive' (op. cit., p. 135). Instead of ultra-metamorphism, then, Sederholm 'prefers the name anatexis which corresponds to refusion although in some cases re-solution would be more appropriate' (op. cit., p. 135). This 'anatexis of the crustal granitic masses has been caused mainly by the influence of emanations from the abyssal magma. . . .' (op. cit., p. 135).

In the foregoing pages I have given a number of quotations from Sederholm's writings which I believe are a fair sample. I have made at various times a fairly detailed examination of his works. I have unfortunately, however, never been able to make up my mind as to how rigidly he held to the view that granitic magma was necessary in migmatization. I consider that I can detect, and that without bias, certain leanings towards the Swedish 'lateral secretion' view in spite of the strenuous debates with Holmquist and other Swedes on this very matter. I present the leading points in this Finnish-Swedish discussion in a later page; now I desire to comment upon the general aspects of Sederholm's work of interest to us on this present occasion. He had dissected, not only petrogenetically but also stratigraphically, the immense complexities of the Finnish Pre-Cambrian. He had demonstrated there an enormous extension of the phenomena made familiar—to those who would listen—by the French school. He had proposed a regional re-fusion of deeply buried portions of the crust—re-fusion connected with granitic magma. In his later studies he stressed the importance of soaking in migmatization and described the results of processes which in my opinion come within my restricted definition of granitization. At this stage of his work, too, he emphasized the importance of metasomatism in migmatization. It is not ungenerous to suggest that he had been influenced in his later developments by the 1921 work of V. M. Goldschmidt on injection-metamorphism in the Stavanger district of Norway, work in which metasomatism found a prominent place. We may examine the pertinent points of Goldschmidt's contribution.

Having concluded his classic studies on the contact metamorphism of the Oslo region, V. M. Goldschmidt proceeded in 1911 to investigate certain problems of regional metamorphism in Southern Norway. Between 1912 and 1921 the results appeared as five parts of his *Geologisch-Petrographische Studien im Hochgebirge*

des Südlichen Norwegens (*Videnskapsselskapets Skrifter, I. Mat. naturv. Klasse*; 1912*a*, No. 18; 1912*b*, No. 19; 1915, No. 10; 1916, No. 2; 1920, No. 10). All these are worthy of profound study by the student of metamorphic geology, but it is the fifth part, *Die Injektionsmetamorphose im Stavanger-Gebiete* (*Vidensk. Skr.* 1920, No. 10, 1921) which particularly concerns us at present. Goldschmidt describes in the Stavanger area a kind of contact metamorphism which is added to the regional metamorphism. The typical rock of regional development is a quartz-muscovite-chlorite-phyllite and this is transformed through intermediate stages marked by the incoming of biotite and garnet into albite-porphyroblast schists and augen-gneisses. These coarse gneisses with large augen of alkali felspar are demonstrated to be the result of the 'injection metamorphism' of pelitic rocks during which there has been an influx of silica, soda and lime into the host. Goldschmidt compares his results with those obtained in other granite contacts, especially the French. Of particular interest to us now, however, is his discussion of metasomatic processes which take place in silicate rocks (1921, pp. 132–7); Goldschmidt (1922) elaborated this discussion in a separate communication. Metasomatism changes the composition of metamorphic rocks through the agency of imported substances—gases, aqueous solutions or melts. One of the most important branches of this metasomatism is that brought about by alkali-transfer; during this alkali-metasomatism, the alkalis may be changed as in the formation of myrmekite, or they may be fixed by the excess alumina in the original rock as in the formation of felspar in injection-complexes and illustrated at Stavanger, or they may be fixed by ferromagnesian minerals or by quartz (Goldschmidt, 1922, p. 110). I have introduced Goldschmidt's work at this place in order to record the foundations on which much of the later proposals has been based.

I have already mentioned the debate between Sederholm and Holmquist on the origin of the 'veins' in the 'veined gneisses', and I propose to look into this in a little detail. We may recall Sederholm's position; the veined gneisses are 'formed by injection of granitic veins' (1907, p. 95) and 'the aplitization of older rock masses which plays such an important role in the formation of the veined gneisses, can in no case be explained, in the present region,

as due to an ultra-metamorphism independent of the eruption of granites, every aplite or granitic vein (being) genetically connected with some special granite . . .' (1923, p. 150). Pursuing his pathological bent, Sederholm calls the veined gneisses *arterites*—the rocks have become filled with new blood. In an arterite or arteritic migmatite 'a schistose rock has been subdivided especially along the planes of foliation' (1926, p. 136). The arterites result from fine injections into pre-existing rocks and Sederholm 'cannot therefore share the opinion of Holmquist, according to whom many of the veined rocks . . . should be regarded as *venites*, i.e., as rocks where the veins should be mainly exudates from the surrounding rocks' (1926, p. 137).

The Swedish view has been put forward especially by P. J. Holmquist (1920, English Summary, p. 209, 1921). Holmquist held that the veins of the veined gneisses were unrelated to granitic intrusion, but were the result of the inner deformation of rocks during regional metamorphism. He admits a local granitic veining, but the veined gneisses are so widespread and their structure is so fine and regular than an origin by injection is excluded (1921, pp. 612–3); the presence of an igneous body simply intensifies the process of veining (p. 615). In high-grade metamorphic zones, there thus arises under suitable conditions a 'real fusion of the most fusible rock masses' (1920, p. 210), a pegmatite-palingenesis, not related to igneous intrusion—the veined gneisses resulted from this *ultra-metamorphism*. Holmquist proposed the term *venite* for the veined gneisses, this term having no genetic significance; there are lateral-secretion venites such as those formed during ultra-metamorphism, and there are injection-venites which correspond to Sederholm's arterites.

To the onlooker, much of the debate between Holmquist and Sederholm seems rather academic. Holmquist (1921, pp. 627–8) considered that in the areas where folding and magmatic injection are coeval, the conditions for a pegmatite vein formation are ideal —high pressure, high temperature and abundance of mineralizers. Changes of a diagenetic, metasomatic, pneumatolytic, palingenetic or magmatic nature are the consequences (1921, pp. 630–1). Holmquist admits that granitic magmas may develop from the formation of venites whilst Sederholm admits an anatexis especially at great depths. As Sederholm (1926, p. 137) has himself remarked

'the difference in principle is not very great between these conceptions'. He 'thinks that the granitic masses, which have been mobilized already at a very great depth, and their emanations, invade the solid rock masses rising from below, when their vanguard of pegmatitic veins may advance even long before the great army, in some cases joining with revolutionary forces of the country invaded. To Holmquist, the granitic veins of the mixed rocks are in large measure due to the action of such revolutionists, acting alone, on their own initiative, and often having little direct connexion with the invaders' (Sederholm, 1926, p. 138). It is important to realize, however, that Holmquist's study of the veined gneisses was an important contribution to what we now call metamorphic diffusion and metamorphic differentiation and also to the idea of selective refusion—all these are notions which have been developed by later Fennoskandians such as Eskola, Wegmann and Backlund.

Certain parts of the ground in South-West Finland made famous by the researches of Sederholm have been worked over again on modern tectonic and petrological lines by C. E. Wegmann and E. H. Kranck. Such of the results of these workers as have reference to granitization in its broadest sense may be noted here; they are extracted chiefly from the 1931 memoir, 'Beiträge zür Kenntnis der Svecofenniden in Finland, I, II', *Bull. Com. Géol. Finlande*, No. 89, 1931. Wegmann concludes that the greater part of the Hangö granite was not a molten silicate mass in the usual sense of the word, but a mass soaked with granitic solutions coming from below. Where this soaked mass moved and met with enough resistance at the walls it presents to us the characters of an *intrusion*; movement gives an intrusion, no movement gives an area of granitization. The pegmatite dykes are that part of the granitizing solutions which crystallizes in joints and opened spaces arising in the permeated mass (1931, pp. 58–9). Around the *colonnes filtrantes* lies a mantle with different kinds of enrichment in different parts; in the inner parts is an enrichment of Si and K, yielding a quartz-felspar enrichment, in the outer is an enrichment in Mg in a cordierite-rich zone, whilst still further outside is an epidotized zone of unakites (loc. cit., p. 62). Since material is brought in at one place it must disappear at another place, and a whole series of chemical readjustments are set in motion. The

solutions strive in the direction of the gradient of chemical concentration, but they are hindered by the small permeability, the fall in temperature and the formation of insoluble combinations in rocks capable of absorbing the necessary contributions (loc. cit., p. 63). Wegmann relates these events to orogenic movement, this style of movement and atomic movement going hand in hand (loc. cit., p. 64).

Wegmann is a tectonician, trained in the school of Argand. His kinetic schemes are translated into petrological processes by Kranck who considers that in the case of the Hangö granite, solutions permeated along movement-planes and spread out from these (loc. cit., pp. 92–3). In the granitization zone typical minerals are cordierite and anthophyllite, farther out are lower temperature minerals like andalusite, kyanite and garnet, whilst at still lower temperatures appear staurolite and finally tourmaline. The whole process is one of exchange of minerals rich in MgFe(Ca) against KAlSi; a magnesia metasomatism, associated with the formation of a potash-granite (loc. cit., p. 94). Kranck concludes that migmatization is not the result of a melting due to granite intrusion but arises through a metasomatism brought about by pegmatitic solutions, the ichors of Sederholm; granitization is a metasomatism rather than a migmatitic intrusion (loc. cit., p. 96). In later studies Kranck reaffirms this conclusion that granitization is not an injection of granitic magmas, but an injection of gases or watery solutions which lead to a pure metasomatism whereby transport of material goes on without marked mechanical alteration in the rock affected (e.g., 1937, p. 85). The position of Sederholm, especially so far as the presence of a granitic magma is necessary for migmatization is concerned, is becoming undermined by operations in his own citadel.

After these detailed studies in Finland and associated with some spectacular work in Greenland, which I note later, Wegmann (1935*a*, 1935*c*, 1936) was moved to discuss general problems of the migmatites. According to Wegmann, the occurrence of migmatites is of two types, one in the marginal zone of granites which have been intruded as magma, and the other in a zone where the country-rocks have been transformed into granitic rocks. Since all sorts of sedimentation-textures and tectonic structures are preserved in granite, it follows that the migmatization has passed

like a wave through the country-rocks leaving a granitic and gneiss zone behind as a witness of its passage (1935*a*, pp. 311, 312). The advance of this migmatization front combined with the not coincident advance of the cooling-front leads to the immense variety of phenomena in migmatites (loc. cit., p. 315). Wegmann defines and discusses four terms constantly employed in the study of migmatites; we may deal with these terms at some length.

i. INTRUSION. The entrance of magma (Schmelzflüss) into spaces which it opens.

ii. INJECTION. The spurting-in of solutions, pneumatolytic or hydrothermal in character. The use of the term injection-gneiss for migmatites in general is not warranted—injection must be proved.

iii. MELTING-UP (Aufschmelzung). The country-rock becomes a structureless melt which crystallizes anew. This process is not important in migmatization (loc. cit., p. 318).

iv. MIGRATION OF MATERIAL (Stoffwanderung)—often called metasomatism, permeation or imbibition. The migrating material moves as molecules or atoms through the rock, and reacts with its host in a process usually called metasomatism (loc. cit., p. 319). The migration proceeds by the oil-spot mechanism, no channels being visible (loc. cit., p. 321). Pseudomorphs of the country-rock are formed with preservation of its geometry: there has thus been no increase in volume, so that material has moved out also. If the structures and textures of the country-rocks are preserved there can be no question of melting-up (p. 322); in fact, melting and melting products are most undesirable terms in migmatization and solution and solution products are preferable (p. 323). Wegmann emphasizes the importance of the intergranular film in the movement of material and associated reactions. By integration of the multitude of concentration gradients around the individual grains, there is produced a kind of front in which most changes take place. When the material in the host available for reaction is used up, the front, if the temperature is favourable, moves on. Thus, the relation between supply and cooling decides where the margin of, say, granitization shall be. If the front is stationary there may be a sharp contact which, however, does not mean intrusive contact; as is well known, intrusions can have hazy contacts. Fronts may be characterized by different minerals,

as for example soda at Stavanger, potash at Hangö, magnesia at Orijärvi (loc. cit., pp. 326–7). As one front advances it may cause the advance of another; for example, the formation of a potash or soda front as witnessed by felspathization may push out the unwanted magnesia and give rise to a cordierite front (loc. cit., p. 327). The temperature gradient has a profound influence on the movement of the migmatite front. An increase of temperature increases the movement of the intergranular film, and more movement of material brings in more heat; further, many of the reactions go on with evolution of heat; all these add up (pp. 328–9).

So far Wegmann had concerned himself with movements of the immigrant materials and he next considers movements in the substratum and the migmatite zone. A distinction must be drawn, he maintains, between happenings in the non-migmatitic superstructure (Oberbau) and the migmatitic infrastructure (Unterbau) lying below (loc. cit., p. 332). The two zones fold in disharmonic fashion, and the migmatite zone comes to fill the arches in the superstructure, as is seen in Greenland. Between the two zones is a transitional zone which is the site of the so-called regional metamorphism and may be narrow as at Pellinge in Finland or wide as in the Scandinavian Caledonids. During the pressing forward of the migmatite front, the wave of 'regional metamorphism' of the transitional zone moves forward in sympathy. In fold-belts with great cross-shortening, the superstructure is the site of over-thrusts, the transitional zone of great nappes and the infrastructure of steep flowage folds. Further, since both the upper part of the infrastructure and the transitional zone were both once in the superstructure, it is not unexpected that relics of superstructural characters are found in them (loc. cit., p. 334). The folding of the superstructure is of two kinds, by contraction of the segment or by collection of the infrastructure into arches and its heaving-up. In these compressed arches of the infrastructure, recrystallization is especially intensive; in many cases obvious granite stocks break through the folded gneisses; when these stocks are investigated in detail, however, many of them prove to be the recrystallized developments of strongly folded migmatites (loc. cit., p. 334). By strong folding of the superstructure, tongues of infrastructural type are formed, and extreme kneading of such tongues and their recrystallization convert them

into granitic intrusions in some of which a migmatitic origin is no longer discernible. These tongues of infrastructural nature can form at different phases; when they are overhauled by the migmatite front they are the Early Granites; when they are developed at the maximum of the migmatization and invade the transitional zone they are the Main Granites; when they are formed during the retreat stage of the fronts, they intrude the already solidified part of the migmatite zone and are the Late Granites (loc. cit., p. 335).

The migmatite front is considered by Wegmann to encroach from below on to the base of the superstructure; heat and material flow in and create an infrastructural condition whereby geosynclinal sediments become the host rocks for fresh migmatitic cycles (loc. cit., p. 336). When the temperature and pressure reach certain limits the rocks of the crystalline cores become capable of movement, their contacts begin to move, transport of material by the intergranular film takes place and new migmatites arise in the old basements (loc. cit., p. 338). As the migmatite front advances higher in the sedimentary pile, it cuts across the stratigraphical boundaries, and may develop an arch-like form; the transitional zones, which are the zones of regional metamorphism, thus lie obliquely to the stratigraphical planes (loc. cit., pp. 339–40).

As just noted, Wegmann considers that granitization has forward optimum and backward stages in all of which migmatites press forward into the outer zones and solidify there as granites, early, main, or late. Relations of these granites to the country-rocks differ and depend upon the zone of their intrusion. Investigation in many migmatite areas shows that many granites and granodiorites are of mixed origin; they display relict structures which cannot be accounted for by derivation for a basaltic magma (loc. cit., pp. 342–3). Wegmann (loc. cit., pp. 343–4) holds that according to the original concept migmatites are magmas; this is a matter I discussed at some length in last year's address and I need not pursue it further here beyond recalling that I consider a distinction between magma and migma to be essential for progress in these studies.

The last aspect of Wegmann's general discussion that I wish to notice concerns the source of the invading material. This source is usually supposed to be from associated intrusives. These intrusives admittedly have great contact zones in the transitional zone, that

is in the zone of regional metamorphism, but they do not attain the size of the migmatite areas. Many of these intrusives, moreover, are themselves of mixed origin. It must be confessed that in many regions the source of the invading materials is not yet known. They must come from great depths at which they must occur in quantity. If they arise from deep melts, two possibilities may be entertained: they may be concentrated by a crystallization-differentiation or they may be squeezed out from the depths (loc. cit., pp. 345–6).

Many of Wegmann's views are illustrated in his Greenland work (e.g. 1935*b*, 1938), and there is no need to elaborate them still further here. I have given them at some length because I suspect that the acquaintance of British geologists with the Finnish school has often stopped at Sederholm. It is well at least to know where the latest developments are to be found. We shall note further developments in Fennoskandia in the next section of these remarks, but I now wish to make a brief reference to Niggli's criticisms of Wegmann's views. Niggli (1942, pp. 1–84) contends that Wegmann has enlarged Sederholm's concept of migmatite by admitting many granitization processes in which magma has played no part (loc. cit., pp. 27–8). Further, Wegmann's molecular transfer leads to the formation of uniform rocks in which host and transferred material are not separable—such rocks do not comply with Sederholm's rule that migmatites 'look like mixed rocks' (loc. cit., p. 29). The intergranular film of Wegmann (which Niggli claims as his own invention in *Die Gesteinsmetamorphose*, 1924, pp. 159, 162) really amounts to very little in metamorphism and if one follows Wegmann the whole way then one has to accept practically all metamorphic rocks as migmatites (loc. cit., pp. 29–30). Wegmann is never very clear, according to Niggli, as to whether he is dealing with a granite solidified from a magma or a recrystallized country-rock nor as to how much solution phase is present at any moment nor as to how much is required for activation or mobilization of the material. In the ordinary processes of metamorphism, Niggli protests, an inhomogeneity can be produced by the plasticity of some parts and the rigidity of others, so that, as in the granite-gneiss of the Pennids, the result should be a 'migmatite' (loc. cit., pp. 35–6). This leads Niggli to define the term migmatite and I requote his definition:

'Migmatite should be restricted to rocks and rock-zones which show coarsely variable magmatic and metamorphic structural patterns, originate in the transitional zone between the magmatic and the solid state, and have undergone a metamorphism during which, without any increase of volume, a considerable part attained a fluidal or melted condition. The association of migmatites with a granitic magma is not necessary to the definition nor do the source and history of the fluid phases affect this either. Mixed rocks like Holmquist's venites can thus be accepted. It must expressly be emphasized that rocks with a comparable appearance may be formed which cannot be called migmatites because they did not originate in the border-region liquid: solid within the earth considered as a whole' (loc. cit., pp. 36–7).

Niggli objects, and rightly in my opinion, to Wegmann's treatment of magma and maintains that Wegmann uses magma when migma would be more acceptable (loc. cit., pp. 39–40). Niggli finally restates the magmatist position, and this statement, in my free translation, is worth giving here:

'In summary, we can well say that granite and granodiorite with the usual chemical, mineralogical, structural and textural characteristics have the typical characters of magmatic rocks and that their explanation as normal metamorphic rocks, for example as metasomatically altered sediments, *appears impossible to all petrographers who have investigated a great deal of eruptive and metamorphic rocks in the field and in the laboratory*. The new idea that granite is a metamorphic rock rests on no kind of fundamental investigation. True migmatites are marginal to granite as Sederholm, Grubenmann, Niggli, Holmquist, Barth, Scheumann readily agree. So-called, "granitized" metamorphic rocks must be separated from the granites and granodiorites.'

I regret in no way that I have been so undignified as to italicize a revealing portion of Niggli's statement above. Both Niggli and Wegmann are Swiss; perhaps in their haven of neutrality, they will favour us with further skirmishes.

Another Fennoskandian, Helge Backlund, writing about the same time as Wegmann, put forward somewhat similar notions which he has since developed. According to Backlund (1936) the crystalline rocks, including in these the plutonites and the metamorphites, mark stages in the evolution of the geosynclines—a fact not generally recognized because, firstly, crystalline rocks

are not important in the Alps and second, crystalline rocks are regarded as having been formed at great depths. Backlund believes this relative depth must be small in comparison with the depth of folding and the total thickness of sediments in the geosyncline (loc. cit., pp. 293–4). Sederholm, as we have already seen, held that the older the mountain belt was, the deeper erosion had bitten into it. Backlund shows that Aktualismus is valid in the oldest fold-belts, and that fold-mountains of all ages are capable of being compared. The series of ultra-metamorphic stages or stages of increasing mobility, venite ⟶ augengneiss ⟶ arterite ⟶ migmatite ⟶ palingenite ⟶ diapirite, brought about by the increased activity of a probably alkali-silicate solution (ichor) and an increase in temperature and considered typical of the old fold-chain, is now known from fold-belts of all ages. The changes take place in response to the rise of the migmatite front (loc. cit., pp. 294–6). Admittedly very irregular arrangements are found in the old fold-belts, but it must be remembered that in these no great vertical thickness is visible, whilst in the younger fold-belts no great area of crystallines is exposed (loc. cit., p. 302).

Backlund agrees with Wegmann that in the latter's transitional zone or zone of regional metamorphism, metamorphic zones of the customary type will be recognizable, but in the migmatite front such become lost, distinctions between 'para' and 'ortho' rocks disappear, and the boundary between ultra-metamorphic and plutonic rocks gradually vanishes. As the migmatite front advances, different rocks react in different ways. The migmatization of pelitic sediments is a familiar occurrence; only a small influx of soda and silica is needed to change the chemical composition of a clay into that of a granite; clay rocks are widespread homogeneous rocks fit for large scale migmatization and granitization. But in the geosynclinal piles, quartzites and limestones are likewise abundant, but yet are rare in the migmatite areas. What has happened to those monomineralic sediments? (loc. cit., pp. 309–11). After a detailed examination (loc. cit., pp. 311–340) of various occurrences of quartzites and limestones in migmatite areas in Fennoskandia and Greenland, Backlund concludes that they have been transformed into granite and granodiorite respectively by some kind of diffusion process in which KNaAlSi play the chief part (loc. cit., p. 342–3). Migmatites are never

homogeneous melts, but a mixture of strongly preponderant solid phases with a little fluid phase, or pore-magma, to use Eskola's term (loc. cit., p. 321). What we call 'rise of magma' is the same thing as the rise of the migmatite front. Elements with small atomic radii, such as Al and Si, form the spearhead of the attack, the alkalis, especially K, follow later; Early Granites are accordingly poor in alkalis, Late Granites are rich in potash. This 'rise of magma' is granitization. During it, the volume-changes of the different types of sediments form a series increasing from pelite through quartzite to limestone. Because of this, original pelites on migmatization remain closest to their original position as paragneisses, whilst derivatives from the quartzites and limestones become orthogneisses, move into the higher parts of the fold-mountains and, at still higher levels, intrude as true magmatic rocks (loc. cit., p. 343).

Thus, for Backlund, all the so-called magmatic rocks (except the original basalts and their direct differentiates) are developed from geosynclinal sediments by granitization; these products may arise *in situ* and then become plutonites, or they may come up from below to give the diapirites, piercing through the original cover (Backlund, 1937, p. 239). These views of Backlund, as may be expected, caused, and still cause, a considerable amount of criticism among other Scandinavian workers; his major debate with von Eckermann forms the subject of the next section of this address; but I wish to refer first to his general statement (Backlund, 1938*b*,) of the granitization theory, which he issued in reply to his critics, especially N. H. Magnusson (1938). Backlund considered that granitization includes such processes as migmatization (loc. cit., p. 179) and anatexis (loc. cit., p. 181) and now applies it to the formation of the older Finnish granites, though originally Sederholm only demonstrated its occurrence in association with the emplacement of the younger Pre-Cambrian granites of that area. Among the evidence described are the presence of relict sedimentation- and folding-structures in these granites, the regional orientation of series of xenoliths, and their relations to dyke-intrusions. It is true, says Backlund, that such younger granites as the Hangö granite are accompanied by great developments of pegmatite (and thus Sederholm's concept of the ichor or granitic juice is understandable), whereas the older granites

lack pegmatites. This is explicable if the granitizing matarial, the emanations, though probably of the same composition, met different kinds of country-rocks. In the case of the older granites the country-rocks were poor in alkalis and so completely used up the permeating alkali-rich fluids; but when the younger granites were formed the country-rocks had already been felspathized so that there was an excess of ichor which appears as pegmatites. Ichors, in Backlund's opinion, cannot however be identified with pegmatite (loc. cit., pp. 181–2).

Turning to the great Scandinavian granites, shown on the maps by continuous areas of uniform colours, Backlund remarks (loc. cit., pp. 186–7) that these 'granite' masses are in reality very heterogeneous, depending upon the heterogeneity of the sediments which have been granitized. He rightly insists on the importance of the room-question in the interpretation of the older Pre-Cambrian. The rocks are usually folded and compressed; emplacement of granite in such a milieu can only be replacement; for a reasonable answer to this room-question 'scheint ein Granitisationsakt der einzigen Weg zur Lösung zu gewähren' (loc. cit., p. 187–9).

According to Backlund, as we have seen, granitization *in situ* (i.e., granitization in my sense) causes an increase in the amount of material in a given space; the result is that a *mobilization* or *rheomorphism** of the granitization products takes place. This mobilization leads to flow-movements, differential movement, flow-folds, penetration and intrusion in the direction of least resistance (loc cit., p. 190). Granitization is controlled by the capacity of the country-rocks to allow diffusion both as regards quality and quantity of material. Diffusion will increase with temperature and will depend also upon textures, such as schistosity, and upon the make-up of the sediment. Granitization thus may become the culmination of metasomatism and find its expression in rheomorphism leading to sharp contacts (loc. cit., pp. 191–2). Backlund prefers *emanation* as a neutral term to ichor and granitic juice, which denote more or less directly residuals from the

* 'Der Verfasser schlägt hiermit vor, unter der Bezeichnung 'Rheomorphose' die gesamten Prozesse der thermalen, partiellen oder gänzlichen Verflüssigung eines praexistierenden Gesteins unter Zuschuss von kleineren oder grosseren Mengen, auf Diffusionswegen zugewanderten neuen Materials zusammenfassen' (Backlund).

crystallization of a granitic magma, since in many cases granitization has clearly been caused by contributions which are not in the least related in any character to such residuals (loc. cit., pp. 195–6). Finally, he emphasizes the selective nature of the action of the emanations (loc. cit., p. 196).

We saw in an earlier page how the French School came to fruition in the work of Michel-Lévy and Lacroix; we have now seen how the work of the Fennoskandians, Sederholm, Holmquist and others, has flowered in that of Wegmann, Kranck and Backlund, though it is perhaps true that many of the later blossoms would have appeared exotic and some even monstrous to their early planters. Let us get a broad view of these latest Fennoskandian developments as they will undoubtedly influence the future course of the granitization theory. Sederholm's requirement of an igneous granitic magma coming from below and acting as one parent in migmatization has to a great extent been set aside. Granitization, as I use the term in this address, has become the most important aspect in migmatization. It is considered to have been brought about by the metasomatic activities of emanations of no specially defined origin, which have diffused up into higher levels of the crust. Their work is helped by an intergranular film, pore liquid or pore magma, and they are able to transform great masses of sediments into granitic and migmatitic rocks. Under certain circumstances of concentration or of orogeny, the granitized rocks are capable of becoming mobilized, of moving and of acting as intrusive rocks. They can thus rise into higher levels of the crust and lose contact with their genetic associates. The process of granitization and migmatization advances in wave fashion and through the rocks 'fronts' of different qualities move at different speeds with different results; if anything is brought in, something must move out. The movements of the migmatite front, with the generation of granitic rocks and the impression of regional metamorphism on great masses of rocks are correlated with tectonic events in considerable sectors of the earth's crust.

These latest Fennoskandian results, right or wrong, are immensely stimulating. They cannot be ignored, and if they are not accepted they must be combatted with vigour. I have now one further contribution from Fennoskandia to consider, but it is of sufficient interest to merit a new section of this address.

V. RAPAKIVI

At various places in my examination of the Fennoskandian contributions given in the previous section of this address, I have mentioned that most typically Fennoskandian rock, the rapakivi granite, and have indicated the existence of profound differences of opinion concerning it amongst Finnish and Swedish geologists. I propose now to give some account of this discussion since its prime topic is whether this celebrated rock is a granite in the orthodox sense or a product of granitization. This account, too, will serve as a fitting preliminary to the examination of felspathization which follows.

The rapakivi granites (*rapakivi*, Finnish, *crumbly stone*) make very large rounded masses in South Finland, a major one on the east side of Lake Ladoga, a second in the Viborg area to the west of this lake, and a third in the Pori region on the east side of the Gulf of Bothnia; the Åland Islands are dominantly of rapakivi, and other extensive developments are seen on the west side of the Gulf of Bothnia and elsewhere in Sweden and in Norway. The typical rock, the rapakivi granite proper, is characterized by abundant large ovoids usually about 4 cm. across of potash-felspar which lie in a matrix of quartz, potash-felspar, plagioclase and dark mica and, sometimes, hornblende; often the potash-felspar ovoids are mantled by a ring made up of small oligoclases, and it is this striking phenomenon which is usually in mind when the rapakivi granites are considered. It must be mentioned, however, that many of the granitic rocks geologically associated with the rapakivi granites proper do not show these mantled ovoids. Further, although Sederholm (1932, p. 24) held that the rapakivi granites of Fennoskandia were of one general age and quality, it would be stifling the whole discussion as to their origin not to admit the occurrence both in Fennoskandia and elsewhere of granitic rocks, with rapakivi textures, of other ages. The group of rapakivi granites occurs in Fennoskandia in association typically with the Jotnian Sandstone or its equivalent or with some other sandstone; Backlund. (1938*c*, p. 347) has said 'it has become a rule in Fennoskandia, when encountering a Jotnian sandstone

area to search for rapakivi or their effusive equivalents in the neighbourhood'.

For most people, of course, the rapakivi granites are igneous rocks, consolidated from a magma; some petrographers, for example A. Johannsen, admit they are 'abnormal granites' and considerable discussion has been concerned with their 'abnormalities', especially those of the felspars. Any reader of this address who has available Sederholm's memoirs on migmatites will find an account of the rapakivi granite of the Pellinge district, illustrated with excellent plates, in Memoir No. 58, pp. 75–95. As I have been fortunate enough to have seen something of this occurrence, I propose to select from Sederholm's description certain points for remark. A contact of the granite, here with typical ovoids, is well exposed on a small island south-west of Buckholm in Perna and has been described and figured in detail by Sederholm; the point of interest to us is that ovoids occur in the country-rock, a meta-andesite, for a short distance from the granite. This is not an isolated occurrence and many examples are supplied by Sederholm (e.g., 1923, Figs. 28, 39, 40) and others. These phenomena prove, according to Sederholm (loc. cit., p. 88) 'that the schistose rock has been permeated by the granitic magma'. Again, he holds (loc. cit., p. 88) that 'no geologist could visit these localities without being convinced that the granitic magma is able intimately to permeate neighbouring rocks'.

Before commenting upon these statements, we may look a little closer into the mantled ovoids of felspar which give the real rapakivi its 'abnormal' character. These ovoids are very remarkable bodies indeed. Some are unmantled and consist of perthitic microcline often with a border zone containing idiomorphic quartz. The mantled ovoids show a polysomatic ring of oligoclase, allotriomorphic against the internal potash-felspar and idiomorphic outside. Occasionally the very cores of these mantled ovoids are composed of groundmass minerals which are surrounded by larger potash-felspars and then comes the plagioclase rim. The potash-felspar ovoids may contain concentric rings of inclusions of all the minerals of the groundmass. Quartz-felspar intergrowths of various kinds are common in association with the ovoids (Backlund, 1938*c*, pp. 366–8). It is clear, even from this skeleton statement, that if the mantled ovoids have arisen through processes of

magmatic crystallization then these must have been, like Sam Weller's knowledge of London, 'extensive and peculiar'.

I need give here only an indication of the proposals which have been made by magmatists to interpret these ovoids; a full discussion of the problem has been presented by Sederholm (1928, pp. 83–96). We may here with advantage glance at the complexity and variety of these proposals, remembering all the time that rapakivi ovoids occur also in the country-rocks. P. J. Holmquist (1901) thought that the ovoid shape resulted from the formation of drops of magma out of which each felspar crystallized—the shelled arrangement was still not explained. According to B. Popoff the shelled texture was due to a coating deposited when the orthoclase kernel had sunk into a new environment—repeated sinkings and coatings gave rise to the complex ovoids. Vogt held that the ovoids represent eutectic mixtures of orthoclase and oligoclase, and Harker (1909, pp. 267–8) sees in them the remarkable effects of the supersaturation of the magma with orthoclase and oligoclase alternately. Wahl suggested that the rapakivi crystallization had taken place in two stages; the early orthoclase crystals were more or less completely re-fused into drops by reduction in gas-pressure. I admit that I am not competent to follow Wahl's explanation, but I can at least agree with Sederholm (1928, p. 91) that it is 'geologically as well as geophysically very complicated'. Sederholm himself appeals to a high viscosity in the magma, due to the great amount of oligoclase substance present in it.

The proposals given above refer to Fennoskandian occurrences; felspars with rapakivi arrangements, however, are found in many granites and the interpretations of these add to the variety. Thus rapakivi felspars occur in the granite of Trégastel, Côtes-du-Nord, described by Thomas and Campbell Smith (1932, p. 291); these authors state that 'there can be no doubt that the cause of the rapakivi structure lies in this basification of the liquid phase of the granite magma existing between the already formed prophyritic crystals of microcline. The available potash would be in a measure diminished by the potential formation of biotite so that on continued crystallization an alkaline plagioclase would form on the nuclei of microcline.' Further, A. K. Wells and S. W. Wooldridge (1931) are of a similar opinion, the Jersey mantled felspars

'reflecting the increased basicity of the magma as contamination progressed'.

Backlund (1938*c*, p. 367) has remarked that 'no routine explanation based on the usual conceptions of "magmatic" behaviour satisfies all the implications disclosed by close examination of the ovoids'. Of course, there is no reason why there should not be formidable problems in magmatic crystallization which cannot be explained by the magmatists, though, when present at their councils, I have formed the impression that this possibility had not often been realized. We can suppose, if we so please, that the complexities of the rapakivi texture, though unexplained, are clearly those of a magmatic environment. If that is so, the texture is a witness of a complicated series of events occurring in the consolidation of the melt and requiring a complicated series of physico-chemical controls. Now I would brutally recall once again that similar rapakivi ovoids occur isolated in the country-rocks—an occurrence everyone admits. Are we to believe that the complicated physico-chemical controls operate also in the country-rock environment in exactly the same way as they did in the consolidating magma? We may do one of three things in reply to this question: we may, as is commonly done, happily ignore it; we may, as Sederholm did, talk at large of the permeation of the country-rock by the granitic magma and consider that the differences in environment are in some way reduced; or we may decide that since the country-rock in which the ovoids occur has clearly not been a magma from which they could have consolidated, then we must query the magmatic status of the rapakivi granite itself. We shall see in the next section of these remarks that exactly the same question is raised by all types of felspathization. The rapakivi granites raise it in a more striking but not in a different fashion.

I resume the discussion on the genesis of the Fennoskandian rapakivi granites by a survey of the debate between Backlund and von Eckermann, with Wegmann and others intervening, which enlivened the North in 1937 and 1938. The debate was opened by Backlund in his 'Die Umgrenzung der Svecofenniden'. *Bull. Geol. Inst. Upsala*, vol. 27, 1937, pp. 219–69 (written 15 January 1937), continued by von Eckermann in 'The Genesis of the Rapakivi Granites', *Geol. Fören. Förhandl.*, vol. 59, 1937, pp. 503–24 (MS. received 2 December 1937), stoked up by Backlund in

'The Rapakivi Puzzle, a Reply', ibid., vol. 60, 1938, pp. 105–12 (MS. received 1 February 1938), fanned by von Eckermann in 'The Rapakivi Facts, an Answer to a Reply', ibid., vol. 60, 1938, pp. 113–5 (MS. received 16 February 1938), spread by Backlund in 'Zur "Granitisationtheorie"; Eine Verdeutlichung', ibid., vol. 60, 1938, pp. 177–200 (MS. received 3 March 1938) and blazoned abroad by him in 'The Problems of the Rapakivi Granites', *Journ. Geol.*, vol. 46, 1938, pp. 339–96 (written 12 September 1937). I give the bibliographical details so that any person of low tastes who likes to see the feathers fly may do so.

Backlund used that most exasperating opening, the reinterpretation of another man's ground. The Rätan and Rapakivi granites which von Eckermann had described in great detail in the Loos-Hamra district of Sweden were interpreted by Backlund as granitized sediments of molasse facies (Backlund, 1937, p. 247). Differences between von Eckermann's Swedish granites and the Finnish rapakivis were due to variation in the original sediments in the area of deposition; the Åland Islands rapakivi, intermediately situated, had an intermediate composition. We can form some idea of Backlund's arguments by von Eckermann's immediate and forceful reactions.

Von Eckermann (1936) had shown that the Jotnian igneous rocks formed a magmatic assemblage with its most acid members genetically connected with its most basic and that the Rätan granite was a forerunner of this magmatic province. Backlund's proposals, of course, broke up the unity of this assemblage; the Rätan and rapakivi granites were not magmatic and had no connexion with the basic Jotnian eruptives. Von Eckermann (1937, pp. 504–6 and pp. 517–20) leads off with the devastating counter that his fieldwork conclusively demonstrates that the rapakivi granites and associated rocks weather and disintegrate to make the basal arkoses of the Jotnian sediments; according to Backlund such sedimentation-contacts are misleading—they really represent the limit of the migmatization front rising from below. Von Eckermann follows up by some fast chemical work; the graphing of analyses of acid and basic Jotnian eruptives from five widely separated developments shows, in von Eckermann's words (loc. cit., p. 508) 'the utter improbability of the granitization of anything as unhomogeneous as the basal Jotnian sediments conforming at five

far-removed localities to the strict laws' that govern 'true magmatic differentiation sequences within a magmatic province': further, by comparison of the chemical compositions of rapakivi granites and of the sediments from which Backlund considers they have been formed, von Eckermann shows that 'if we accept the granitization theory, we are forced to assume a mystic hydrous alkaline fluid, partly very rich in fluorine and titanium' (loc. cit., p. 513). This fluid, as calculated by von Eckermann, is considered by him to be quite unlike that established as active in the granitization of the Archean. I feel compelled to interject here that in my opinion this last statement of von Eckermann's is rather wide of the mark. Von Eckermann holds (loc. cit., pp. 514–5) that the rapakivi texture is 'a typically magmatic one' and that 'in magmatic rocks retrograde felspar-crystallization is no rarity'; the peculiar features of the ovoids arise from 'disturbed phase equilibrium (concentration, pressure, temperature) in the course of magmatic crystallization' (loc. cit., p. 515). In such undisturbed, non-metamorphic, sediments as the Jotnian sandstones it should, von Eckermann holds, be possible to obtain 'records within the surrounding rocks of either the transforming solutions, or their roads of transport'—such records are not forthcoming (loc. cit., p. 515). Further, rapakivis are known with roof or floor of Archean rocks; are we to suppose in this case, asks von Eckermann, that the Jotnian sandstone parent of rapakivi was deposited in a cave in the Archean? (op. cit., p. 516). How can Backlund account for the chilled edges of rapakivi porphyries and the occurrence of such rocks well below the horizon of the Jotnian sandstones? (loc. cit., p. 530). Moreover, the Rätan granite shows typically intrusive contacts (loc. cit., pp. 522–3). Finally, von Eckermann inquires as to what was the origin of all the sedimentary material which has been changed into granites, and as to where have come the mystic fluids that did it. Though granitization may be valid in other fields than the Jotnian, he considers it (loc. cit., p. 524) 'a waste of time to build theoretical granitization structures on the quicksands of the Jotnian'. The first round, I consider, goes to von Eckermann on points.

Backlund opens the second round (1938*a*) by a disarming talk on temperament, and follows by a statement on what he considers his task to be. This is, first, to demonstrate that granitization is not a process confined to remote periods and great depths but is

one appropriate to all fold-belts, second to direct attention to the biggest problem in regional geology, the space problem, and third to present some idea of the qualitative and quantitative aspects of the granitization process. He maintains that his studies, including those of von Eckermann's ground, show that Sederholm's ichor could not represent the granitizing matter and that this must have varied with the rocks undergoing granitization; emanations is a better term to use than ichor and the mechanism of its migration has been suggested by Wegmann and others, as we have seen. The demonstration of these three points has been Backlund's purpose in his studies. He proceeds to show that von Eckermann's chemical calculations for Jotnian granitization are exactly in line with those for Archean granitization; von Eckermann's calculations, indeed, 'merely prove that [Backlund's interpretation] is entirely consistent with the results of detailed research elsewhere' (loc. cit., p. 108). This is, in my opinion, a true statement. Backlund (loc. cit., pp. 109–110) allows his thesis to be broadened; rapakivi-like granites occur in the earlier Pre-Cambrian and are admitted to be formed from sediments not of the molasse facies; rapakivi granites may be derived from Archean crystalline rocks,— 'caves' of Jotnian sandstone are not necessary and some rapakivi granites were not necessarily formed from sediments. He insists that the supreme problem is that of space; granitization solves this problem, whereas an appeal to a gigantic magma in which the operations of magmatic differentiation may proceed is contrary to geophysical evidence and, likewise, an appeal to the derivation of the great rapakivi granites by differentiation from basic magma is contrary to geological evidence (loc. cit., pp. 110–1).

Backlund concluded by thanking von Eckermann for the new facts which he had provided, and thus possibly gave him an opening for his reply (von Eckermann, 1938, pp. 113–5). Von Eckermann opens in dashing style with the remark that one man's puzzles are another man's crystal-clear facts. He denies that any space problem exists for the rapakivi granites as they have domed their roof, or have been emplaced by areal intrusion or by block adjustments. Moreover, in von Eckermann's own area, the greatest space problem is that of the basic rocks which does not appear to bother Backlund at all. The basic pole of the rapakivi series is present as the Jotnian diabases in sufficient quantity to satisfy

the requirement of derivation of the granitic from the basic magma. The alternation of horizontal, supposed granitized, sediments with basic eruptives and completely non-metamorphic arkoses is so far unexplained by Backlund and until this and similar problems are solved, von Eckermann does not propose to take 'camouflaged rapakivi-sediments' seriously.

In a previous paragraph I noted the view of Backlund that the granitizing material must have varied with the rocks undergoing granitization. Von Eckermann (loc. cit., p. 114) seizes upon this and says 'he (Backlund) gives no explanation of the geophysical laws which allow the different sediments to order by special delivery from the ascending migmatization-front their own exclusive transmuting emanation'. He doubts that sediments have 'such discriminating ability in choosing a suitable metamorphosing partner'. This statement reveals, in my opinion, that von Eckermann has completely missed one essential point about the granitization process. Von Eckermann's criticism has been well met by MacGregor and Wilson (1939, pp. 207–8), who say: 'During metasomatism such rock types as are already saturated for one or more components will not show any reaction to those particular components should they be present in the penetrating medium. Different sediments *will not have* to order by special delivery from the ascending migmatization front their own exclusive transmuting emanation. Rather they take their pick of a selection brought around on approval, and give in exchange equivalent amounts of those commodities which the new purchases are expected to replace.'

During the Backlund–von Eckermann debate, a general paper by Backlund had been awaiting publication in America. In this paper, 'The Problems of the Rapakivi Granites', *Journal of Geology*, vol. 46, 1938, pp. 339–96, there is a good presentation of Backlund's thesis specially suitable for readers not particularly well informed in the details of Fennoskandian geology. There is no need here to summarize the contribution and I present only two aspects. The first concerns the influence of the structure and textures of the Jotnian sediments on the rapakivi granite resulting from their replacement. Diffusion of the emanations, Backlund proposes (1938*c*, pp. 392–3), would be controlled by the bedding planes and the isothermal planes, both controls are horizontal

so that the resultant rapakivi granites are flat sheets. The porosity of these sediments of molasse facies facilitates permeation and the formation of rather homogeneous bodies of replacement origin. The original sediments are bedded and graded; sharp boundaries to the rapakivi are produced by the influence of this grading, whilst traces of the original bedding are preserved in the granite as a series of equidistant horizontal planes characterized by mineral concentrations different from those of the general rock. Patches in the rapakivi crumble due, Backlund supposes (loc. cit., pp. 358–9, 393), to an original variation in permeability so that certain patches of the sandstone were, so to speak, 'unwetted' by the emanations, and thus did not receive their final 'cementing' material—such patches accordingly were not transformed into really good rapakivi. Secondly, in this general paper, Backlund (loc. cit., pp. 391–2) details the course of the transformation of the arkose into rapakivi granite; it is controlled by fluctuations first in temperature and second in the chemical nature of the metasomatizing emanations. Crystallizations, intergrowths, resorptions, replacements and reorganizations become possible so that the complexity of texture of the rapakivi granites results.

To the onlooker outside the ring it appears that during this debate Backlund permitted certain portions of his thesis to be considerably widened and generalized whilst von Eckermann had maintained fairly intact his Loos-Hamra position. One of the many difficulties that confronts the non-Fennoskandian inquirer in this matter follows from the employment of rapakivi in a geological sense as indicating certain granitic rocks late in the Pre-Cambrian sequence. With these geological implications I cannot here be concerned; for my part, I have simply to remark that such of the Finnish rapakivi granites as I have seen in the field are on the field evidence reasonably interpreted along the lines of Backlund's granitization proposals—such granites, I hasten to say, were not seen by me in contact with Jotnian Sandstone but with metamorphic rocks of one kind and another. I consider we should distinguish two problems; the first is concerned with the origin of the rapakivi rocks from the Jotnian sediments at a given time in geological history, and the second with the origin of granitic rocks having characters like the rapakivi rocks of Fennoskandia. Whilst the first problem is rightly capable of arousing violent

passions in Fennoskandia, it is the second problem which more closely and calmly concerns us now. What of the rapakivi-like rocks of any age or place?

We have seen that Backlund agreed that rapakivi-like rocks occurred of various ages and with varied associations. In the discussion of the genesis of such rocks I have opportunity only to dwell upon the relevant work of Wegmann in South Greenland (Wegmann, 1938). Wegmann (loc. cit., pp. 98–121) found that many granitic massifs in Southern Greenland are surrounded by a zone of rapakivi granite which is transitional into the metamorphic country-rock, gneiss, amphibolites, &c. The rapakivi-type does not occur without the presence of fragments of country-rock; this association gives a structure like Sederholm's agmatites or eruptive breccias, and Wegmann speaks of an agmatitic front. He says (loc. cit., p. 118): 'the rapakivi texture occurs in the agmatitic front, which forms a shell round the syenitic massifs'. He describes and figures beautiful examples of xenolith-rich rapakivi granite with rapakivi felspars developed in the xenoliths. He concludes that all the field-evidence 'speaks in favour of a metamorphic origin' of the rapakivi texture. The previously existing rocks have been transformed into rapakivi-like rocks by the advance of the agmatite front, 'such agmatites are found when the roof is much jointed or in other ways contains beaten tracks along which the volatile components may prepare for action' (loc. cit., p. 117). Wegmann cannot agree with Backlund in his original view that molasse sediments are necessary for rapakivi formation as he finds rapakivi granite produced in Greenland from 'very different original rocks of very different ages' (loc. cit., p. 117). Nor can he agree with Von Eckermann with regard to the supposed absence of pegmatites from the rapakivi granites; he says 'pegmatites are not absent from the Rapakivi, but the pegmatitic material forms part of this rock' (loc. cit., p. 120).

It is now therefore reasonably well established, in my opinion, that rapakivi-like granitic rocks may arise by a variety of felspathization. We have already briefly noted evidence for this view from Finland and from Greenland and we shall obtain more in the next section of these remarks. I cannot forbear, however, to present one further piece of evidence from Unst in the Shetlands which, fittingly enough once Norse, is now geologically mine.

From Herma Ness in Unst, L. J. D. Fernando (1941) has described 'augen granites' with large mantled rapakivi felspar ovoids (loc. cit., Plate 7, Fig. C). Fernando concludes from his detailed investigation that 'the rapakivi texture, at least as far as the Herma Ness rocks are concerned, is intimately connected with replacement associated with the general process of felspathization. It is evident that the rim of plagioclase is clearly a replacement rim, and the corrosion of the potash felspar is just as evident' (loc. cit., p. 123); 'these rapakivi rocks represent arrested stages in the replacement of potash by soda' (loc. cit., p. 128). Many examples could be produced of the formation of rapakivi-like felspars in xenoliths. I would remind you of my query of an earlier page: Are we to believe that, as illustrated in the formation of rapakivi texture, complicated physico-chemical controls operate in a country-rock environment exactly in the same way as they do in a magmatic environment? For some examples, the answer is clear—there are not two environments, but one environment. We look into this answer both in general and in particular in the next section of this address.

VI. BIG FELSPARS AT GRANITE CONTACTS

In our survey of the French School, and especially of the work of Fournet, Delesse, Michel-Lévy and Lacroix, we saw that the expression felspathization was in constant employment from the earliest times to indicate happenings at granite contacts during which felspars were produced in quantity in the country-rocks. Fournet (1847*b*, p. 72) discussed the problem in connexion with the modification of clay-slates by plutonic action. He notes, as we have already seen, that since such rocks contain potash soda and lime it is possible for felspars to arise in them by simple re-crystallization; at times, however, so much felspar is developed that an impregnation with felspar by some kind of capillary action must have taken place. It seems clear that many of the French used felspathization and granitization as almost interchangeable terms; Delesse is explicit on this point. After all, felspars are simply the most noticeable of the newly formed minerals in rocks undergoing granitization. I have already discussed felspathization in this sense

and now I may take up for the most part the consideration of a special topic, namely, the appearance of large felspars in the country-rocks at granite contacts—a topic which I have introduced in the account of the rapakivis. Before I do so, however, there are certain critics of the general thesis to be dealt with and a few unquestionable examples of it to be outlined.

Harker (1932, p. 250) stated that felspathization is 'the supposed impregnation on an extensive scale of metamorphosed rocks with felspar derived from a magmatic source.' He continued: 'This hypothesis dates from a time when the formation of various felspars as normal products of metamorphism was not adequately appreciated', and he held up as a warning to the believers in felspathization the detailed results of Goldschmidt at Stavanger where in the country-rocks adjacent to the granite 'there has been some accession, not of felspar, but of soda and silica' and where, too, 'it appears that the actual diffusion of material does not surpass a very moderate range'. I submit first, that the formation of felspars during metamorphism without transfer is a fact admitted by all; we saw in a previous paragraph that it was admitted by Fournet, the classic protagonist of felspathization, nearly a century ago. Secondly, I submit that these felspathic 'normal products of metamorphism' are not like the felspar-porphyroblast schists and augen-gneiss so common in the migmatite areas. Further, whatever felspathization once meant, I maintain that it does not now mean an impregnation with felspar derived from a magmatic source. In felspathization, material for the formation of felspar may be contributed both by the country-rock and by the introduced solutions, magmatic or otherwise, as we see illustrated immediately. For me, felspathization means that aspect of granitization in which attention is directed to the formation of felspar. As Harker did, Niggli (1942, p. 29) also stresses the fact that an apparent 'enrichment in felspar' may take place without the slightest change in the bulk composition of the rocks. Many gneisses, he states, have the same chemical composition as mica-schists, and whether much or little felspar has developed is a consequence of the physical and not of the chemical conditions.

The answer to this line of criticism was, as we have seen, also given by Fournet—at times so much felspar appears that felspathization *must* have occurred. It will be profitable, I think, to look

into a few simple cases so that we may be sure of the reality of felspathization—remembering that I have defined this as the formation of felspars in rocks undergoing granitization. Of the innumerable examples supplied by the French School I select the classic case described by C. Barrois (1884) from the Rostrenon granite, Côtes-du-Nord. A figure on p. 14 of Barrois' *Mémoire* shows irregular veins of porphyritic granite in micaceous grey-wacke in which are developed numerous large crystals of ortho-clase, 'diséminées au hasard, à la façon des andalousites dans un schiste, au voisinage du granite'.

Our next example of felspathization is V. M. Goldschmidt's demonstration in the Stavanger region. This is a particularly good example as it is described in detail, is backed by analyses, and is illustrated by self-explanatory plates of rock-specimens. At Stavanger the regionally developed quartz-muscovite-chlorite-phyllites are transformed into albite-porphyroblast schists and augen-gneisses. The albite-porphyroblast schists (1921, pp. 81–7) are obviously the result of felspathization, as a glance at Gold-schmidt's magnificent Plates III (Fig. 2), IV (Figs. 1 and 2) and X (Figs. 1 and 2) would show. These schists occur as layers in garnet-mica-schists, as compact independent rock-masses or as stripes in injection-gneiss. They show porphyroblasts of oligoclase-albite, Ab72, in a matrix of quartz, muscovite, biotite and garnet. By the appearance of potash-felspar they pass with advance of felspathization into augen-gneisses (op. cit., pp. 87–99). Con-sideration of the chemical analyses of the whole Stavanger series shows that the albite-porphyroblast schists and augen-gneisses are produced from the phyllites by an influx of alkalis, lime and silica (op. cit., pp. 108–15). The progress of this influx is revealed by the increase in plagioclase felspar from some 10–12 per cent. to 30 per cent. This felspathization is a typical metasomatic process.

For our next example of felspathization and granitization we may consider that provided by G. H. Anderson from the Inyo Range (1937). Anderson shows that the Mesozoic batholithic granites of the Sierra Nevada are represented by two strikingly different types; one, the Boundary Peak granite, is a white uniform biotite-granite, with nothing about it that would suggest that it 'was formed in any other way than by crystallization from a magma' (loc. cit.,

p. 73). The second type, the Pellisier granite, occurs between the Boundary Peak granite and the overlying country-rocks, limestones, argillites and schists; it is a dark-grey porphyritic rock, very variable in texture and composition, and contains innumerable xenoliths. It is considered by Anderson to be formed *in situ* by the recrystallization of the country-rocks with the addition of more or less material from a subjacent magma—the Boundary Peak granite being the source of the emanations. It is worth while giving a summary of the arguments advanced for this granitization origin; they are (loc. cit., pp. 45–7):

(i) Similarity in composition and texture between the Pellisier granite and the partly recrystallized and replaced rocks of the contact zone; (ii) gradational contacts between the Pellisier Granite and the older rocks; (iii) digestion of xenoliths observable in the granite; (iv) variability in texture and composition; (v) its 'stratification' structure which can, in places, be traced into similar structures in xenoliths or country-rocks; (vi) the grey colour of both Pellisier granite and the country-rocks is due to the same cause—dissemination of ferruginous material; (vii) the granite shows an increase of biotite near biotite-schists; (viii) the regional structural orientation is not changed in the xenoliths, and (ix) the Pellisier granite contains sillimanite and staurolite.

Especially important for our present inquiry is Anderson's demonstration of successive stages in the transition from biotite-schist to porphyritic granite (loc. cit., pp. 37–8; Plate V, Fig. 1; Plate VI, and (especially) Fig. 7, p. 37). Along a canyon wall is exposed a section about a mile long displaying this transition and it is of interest to note that Anderson's Fig. 7 of this section resembles Barrois' Rostrenon figure given half a century before. The process at Inyo begins with the formation of large porphyroblasts of potash-felspar, up to an inch across, within the schist, the matrix of which is but slightly altered. The next stage shows the appearance in the schist groundmass of sinuous bands of quartz and felspar along the foliation planes. This replacement proceeds till stringers of schist are distributed through a matrix essentially granitic; the felspar porphyroblasts persist to the end of the process and here and there they are found partly enclosed in the granitic matrix and partly in the schist 'inclusion'. 'In the final stages of the

replacement the schist xenoliths disappear or become indistinguishable, and the rock is, to all appearances, a porphyritic granite with large phenocrysts, which commonly enclose biotite and quartz. A granite of this type is widespread in the northern Inyo Range' (loc. cit., p. 38). Chemical investigation shows that in the granitization process there has been again an influx of silica, lime and soda (loc. cit., pp. 39–40).

Excellent cases of felspathization have been described and figured from Southern Greenland by Wegmann (1938). From his field observations on the development of felspar lenses, augen or porphyroblasts, Wegmann (loc. cit., pp. 45–6) concludes that 'then phenotype porphyritic granite may have different origins, and does not correspond to one single genotype'. His description (loc. cit., pp. 106–8) of the Sydprøven granite is of very great interest for our present purpose. This granite is coarsely porphyritic, and shows a transitional zone into migmatites. 'The transition is initiated by the formation of big felspars in the gneisses and amphibolites, which gradually gather into large clouds. . . . When the transformation proceeds along all routes there remains a swarm of inclusions whose orientation is still retained.' Wegmann (loc. cit., Fig. 59, p. 108) presents a photograph of a rock surface which deserves the closest study; it has the explanation 'The imbibition of the Ketilidian migmatites and transformation into younger granite', which is simply a statement of what can be observed. In an earlier account of the geology of part of North-East Greenland, Wegmann (1935*b*) described the felspathization of quartzite. Nearly pure quartzites show more and more felspar as they are traced into zones of granitization. For the felspar which occurs 'in the texture of the quartzite' it is clearly necessary to assume a migration or impregnation of material by Termier's oil-spot mechanism. 'Where quartzites and schists are interwoven with feldspar they have become gneisses or granites. . . . Not all the material has migrated, but only part of it, viz., precisely the part which rendered the formation of feldspar possible' (loc. cit., p. 23).

Many examples of felspathization—cases which I do not hesitate for one moment to describe as excellent—have been given by myself and my students from Scottish migmatite areas. Thus L. J. D. Fernando (1941) has described and figured augen-granites

and augen-gneisses formed by the soaking of sodic and potassic magmatic fluids into mainly semipelitic country-rocks in Unst. A glance at the two plates which accompany Fernando's account will convince anyone of the reality of felspathization at Herma Ness. A dozen years ago I described the development of felspar augen in semipelitic rocks in the Central Sutherland migmatite area (e.g., Read, 1931, pp. 116–8, Plate II, B and Fig. 8) and recently Y. C. Cheng has investigated the process in detail in North Sutherland (Cheng, 1942, pp. 67–85, especially Figs. 3 and Plate II, Figs. 2 and 3; 1944, pp. 107–54, especially Figs. 4 and 7). Cheng has shown by the investigation of series of specimens collected dead along the strike of beds undergoing migmatization that the production of permeation-gneiss is 'essentially in the nature of felspathization, with plagioclase forming in the present case'—the compositional changes according with those proposed for analogous rocks in Norway and America. Other cases of felspathization are described by Cheng; certain migmatitic derivatives of semipelitic rocks are shown to be due to the 'circulation of potash-rich solutions along foliation planes with the formation of orthoclase from the pre-existing minerals, (but) a part of the potash needed to form the augen was undoubtedly derived *in situ*'. In other semipelitic derivatives he shows, by petrological and chemical investigation, that felspathization has again occurred with oligoclase as the chief product. Cheng doubts whether certain of his eruptive rocks are igneous in the real sense of the word. 'The "igneous" sheets may be simply channels along which large amounts of magmatic fluids have repeatedly been introduced and have reacted with the surrounding country-rocks to form various kinds of granitic rocks.' Almost all the changes observed by Cheng in his migmatites are considered to result from the metasomatic activity of rather attenuated alkaline fluids, and he works out an immense series of readjustments in his rocks consequent on the disturbances of chemical and physical equilibrium in them.

As my last sample of granitization I cite the detailed study by Doris L. Reynolds (1943, pp. 231–65) of hornfelsed Silurian sediments enclosed in the Newry granodiorite. In the first stage of the process, small irregular bodies of trondhjemite (oligoclase-granite) are formed in the hornfels by the fixation of incoming

soda, lime and silica with the concomitant expulsion of magnesia and iron oxide. In a second stage, potash displaced in the first stage is fixed in microcline porphyroblasts and in bodies of granodiorite character. Once again it is demonstrated that the entry of soda, lime and silica initiates a complex series of chemical readjustments which lead to profound modifications of the original rocks.

In these typical examples of felspathization and granitization just presented, I have been careful to indicate many plates and figures for your inspection. Though to 'go and see' would be pleasant and profitable, especially at this time, these illustrations are I consider of sufficient validity to enable the ordinary reader to make up his mind on the reality of these processes. I have made a selection of the abundant evidence—I could have taken examples from dozens of workers from all over the world. Nobody except him who has learnt less and forgotten less than any Bourbon can deny the validity of the conversion 'of solid rocks to rocks of granitic character without passing through a magmatic stage'. We have noted incidentally that many of the early stages of the granitizations described are marked by the development of large felspar porphyroblasts in the rocks undergoing granitization. We have now to consider this phenomenon more specifically and to discuss what I take to be its fundamental significance.

In earlier pages I have noted Fournet's observation of the growth of large felspar crystals in hornstones and Cotta's record of large crystals of orthoclase in diorite fragments enclosed in the Thuringerwald granite. I would remind you, too, of Michel-Lévy's '*dents de cheval*' from Flamanville—the great orthoclase crystals found in enclaves which are identical with those that give the surrounding granite its porphyritic appearance. I recall also the common occurrence of rapakivi mantled ovoids, with all their intricacies, in country-rocks adjacent to the rapakivi granites, as seen especially well in the Buckholm contacts in Finland and displayed very clearly by Wegmann in Southern Greenland. I have already twice asked what seems to me the fundamental question: are we to believe that, as illustrated in the formation of the rapakivi texture, complicated physico-chemical controls operate in a country-rock environment exactly in the same way as they do in a

magmatic environment? The rapakivi felspars raise this question in a particularly spectacular fashion, but the same question is raised by innumerable, and very likely all, porphyritic granites. I invite you when you leave this room to satisfy yourself of the reality of this question by inspection of the great expanses of Shap and Cornish porphyritic granites displayed along Piccadilly, Fleet Street, Victoria Street and most of the opulent streets of London. You will find, in almost every polished slab, dark xenoliths which display a few large felspars apparently identical with those of the enclosing granite. I take the case of the Shap granite for special inquiry.

The small Shap granite in Westmorland has been investigated by Harker and Marr (1891) and later by D. R. Grantham (1928). The granite is characterized by the occurrence of large pinkish orthoclases in a greyish, moderately coarse, matrix in which orthoclase, quartz, biotite and plagioclase are visible. In addition to obvious xenoliths, the granite contains darker and finer-grained rounded patches which were interpreted by Harker and Marr (loc. cit., p. 291) as derived from an underlying more basic magma. In these dark patches are found large felspar crystals similar to those of the granite proper. As Grantham (loc. cit., p. 326) has remarked, these crystals have been taken to be phenocrysts and therefore to prove that the dark patches are differentiation or segregation products of the magma. Grantham (loc. cit., pp. 326–7) shows there is a detailed parallelism in microstructure between the basic patches and the metamorphosed andesitic country-rock and concludes that the large crystals are porphyroblasts crystallizing from solutions introduced into the permeable andesite xenoliths, which themselves become highly metamorphosed and impregnated.

The Shap felspars have been studied by Edmonson Spencer (1938, pp. 111–3) who shows that the optical properties of the orthoclase porphyroblasts in the 'semipelitic' xenoliths are identical with those of the orthoclase phenocrysts in the granite. It seems that there are not two felspars, but *one* felspar.

Spencer (loc. cit., pp. 113–4) passes on to study the large felspars which occur in xenoliths of Malmesbury shale in the Kloof granite of Cape Town, and the xenoliths and granite themselves. His conclusions are worth quoting:

'It is interesting to observe that if the metamorphism of this particular xenolith had been carried to the stage of complete holocrystalline reconstitution, with homogenization of the felspar porphyroblasts, but without actual fusion, the result would have been a rock closely resembling—if not identical with—a porphyritic potash-rich granite' (loc. cit., p. 114).

'The occurrence of potash-soda porphyroblasts in xenoliths and similar metamorphosed rocks under conditions which preclude an origin by direct consolidation from a magma, raises the question as to how far it is justifiable to regard holocrystalline rocks, even of typical granitic appearance, as products of direct magmatic crystallization. The same process which gave rise to the porphyroblasts, if continued for longer periods or at elevated temperatures, might produce holocrystalline granitic rock structures without the mass having even approached the liquid state' (loc. cit., p. 115).

In their account of a Jersey granite, A. K. Wells and S. W. Wooldridge (1931, pp. 197–8) make a reference to the Kloof contacts just mentioned. They note that the modified granite at Sea Point, Cape Town, bears a striking resemblance to a contaminated rock at Ronez, Jersey, 'particularly in respect of the highly idiomorphic porphyritic felspars that become increasingly prominent the more foreign matter has been assimilated'. Wells and Wooldridge found it difficult to believe in some cases that these South African porphyritic felspars 'have not originated in the country-rock rather than in the granite, although it is not easy to provide an adequate explanation of their genesis on these lines'. They *can* believe, however, that the porphyritic felspars in the Jersey rocks are largely of granitic parentage, as 'proved by certain pink crystals zoned with a thin white mantle . . . seen to consist of a potash felspar interior with a surround of oligoclase, thus reflecting the increasing basicity of the magma as contamination progressed'. Readers of these remarks of mine will be able to estimate the validity of this *proof*.

Certain phenomena very like those described by Grantham from the Shap granite were encountered by H. H. Thomas and W. Campbell Smith (1932) in the Trégastel granite and were explained in quite a different way. At Trégastel, basic igneous xenoliths in the granite carry large felspars 'identical with the felspars of the granite' (loc. cit., p. 289). These felspars and

'even composite fragments of granitic material' were derived from the granite (loc. cit., p. 286). Though 'typical igneous structures are preserved in the basic mass throughout all the phases of hybridization . . . a certain degree of fluidity was produced (as) shown by the presence well within the margin of the basic hybrids of xenocrysts of felspar and of actual granitic material (which) must have been derived in their present condition from the granite and introduced mechanically into the hybrid rocks' (loc. cit., pp. 289–90). Thomas and Campbell Smith agree, in this mechanical forcing-in of the felspars, with the earlier opinion of Grenville Cole (1894, pp. 239–48) and Harker (1904, p. 219). They cannot agree for instance with Grantham's opinion that the crystals of orthoclase at Shap have grown within the xenoliths.

This is a matter on which the field-evidence shows there can be no kind of doubt whatever. Records of hundreds of field-exposures show that the felspars *cannot* have been 'introduced mechanically'. In the Trégastel examples themselves, the 'igneous structures are preserved' in the xenoliths. There springs to my mind the case of very large sharp-edged xenoliths of semi-pelitic rock in the Skaw porphyritic granite of Unst in the Shetlands (Read, 1942); in the midst of the xenoliths are large felspars inches across identical with the porphyritic felspars of the granite, but the delicate bedding, displayed often in three dimensions, is not disturbed in the least. A felspar 'introduced mechanically' must in these examples have covered up its tracks pretty well.

The inability of the magmatists to interpret the plain field-facts arises of course from the inertia of a century of belief in the magmatic origin of all granitic rocks. This inertia has been increased for many by the addition of weighty physico-chemical arguments which 'explained' the phenomena. It takes a certain amount of courage to cut adrift from authority and to go round to the other side of these problems and look at them from a completely different direction.

We have seen that it is inherent in the view of the magmatists that, first, porphyritic granites consolidate from a magma and that, second, enclaves containing similar porphyritic felspars must be differentiation or segregation products of that magma. The large felspar crystals become the touchstone of magmatic origin of the

rock containing them. Something might be said for such an argument so long as the enclaves in question were anything like the igneous rocks; for example, Harker and Marr, as we have seen, contentedly derived the dark more basic patches in the Shap granite from an underlying more basic magma. The argument completely collapses, however, when the enclaves which contain the touchstone felspars are unquestionably of sedimentary origin; it has, in fact, to be put into reverse, unless we take refuge in a mechanical introduction for them. It now runs thus: because the enclaves containing the felspars are sedimentary, then what encloses the xenoliths and similar felspars is also sedimentary—the porphyritic granites are produced from sediments by an intensification of the felspathization process. Both ways of the argument depend upon the identity of the felspars, and this has been definitely proved in numerous cases. Is the sedimentary argument any more valid than the now discredited magmatic argument? It is, because though we can demonstrate the sedimentary origin of the key enclaves, no one can *demonstrate* the magmatic origin of the porphyritic granite which contains them. Niggli (1942, p. 6) has made play with the fact that lavas of granitic composition are abundant; this fact does not, however, prove the existence of a granitic magma—it simply records that lavas of granitic composition are abundant.

One can, of course, believe that the two sets of felspars, though admittedly identical, are really formed in two quite different environments, one magmatic and the other solid rock. I am tempted to remark, as the Duke of Wellington did on another occasion, that if one can believe this, one can believe anything! If this is really true, in view of the profound differences in the environments, I feel that a great part of the lore about magmatic crystallization becomes meaningless. I consider it more reasonable, and in the ultimate more honest, to hold that there is only one environment, that of the solid country-rock.

Throughout these discussions we have kept before our eyes the phenocrysts, or the porphyritic crystals, or the porphyroblasts of potash-felspar, and we have done so because these, as their names denote, are the most spectacular elements for observation. We must remember, however, that smaller and less noticeable but just as important elements have to be considered. I refer, for example,

to the appearance of acid plagioclase and possibly quartz in the Shap enclaves (Grantham, 1928, p. 326); these appear, too, in the Skaw xenoliths and have been recorded from numerous other xenolithic contacts. Further, depending upon the nature of the country-rock, a ferromagnesian mineral such as biotite or hornblende arises. With the continuation of the formation of this typical granitic assemblage, the country-rock xenolith is gradually made over to a granite; I recall Spencer's note on the xenoliths of Malmesbury shale in the Kloof granite, for example. To satisfy yourself of this, examine carefully the next *large* polished slab of Shap or Cornish porphyritic granite that you come across. There is nothing mysterious or complex about these proposals provided we look at the matter in the right way.

Any reasonable person will admit, if he takes the trouble only to examine the diagrams and plates illustrating the relevant papers cited in this and the previous section of this Address, that granitization is valid in the great migmatite complexes. We have by now admitted also, I trust, that even in the case of the apparently 'igneous' granites, the 'proper' granites or the 'real' granites as they are often called, granitization may also be valid. Many of these 'real' granites appear to be eruptive, they evoke the idea of eruptivity by their mode of occurrence, 'leur apparition parait soudaine', as de Lapparent would say (1923, p. 79). Granitic rocks formed by granitization can, and indeed often do, evoke the idea of eruptivity—they certainly appear suddenly. We may recall Wegmann's view that where the granitization front is stationary, sharp contacts will be formed which are, however, not intrusive contacts. Circumstances of temperature, pressure or concentration may give rise to knife-edge boundaries to the products of the granitization reactions. Some granites, however, are held to come into place by intrusion. Is it possible for the products of granitization to be made to move or to flow either as a migma or a magma? If this is possible, then there may be a great connected family of granitic rocks—a family including granitization products, migmatites and magmatites. We examine this possibility in the next section of these remarks.

VII. MOBILIZATION

In intrusion there must be movement of material. Wegmann (1935*a*, p. 315), in his discussion of terms associated with the study of migmatites to which I have already referred, defines intrusion as the entrance of magma into spaces which it opens. Admittedly, many intrusions are of magmatic or migmatitic material (however these may be defined), but it is well to remind ourselves that a variety of other rock-materials—rock salt, cornstone, limestone, clay and so forth—can show intrusive relationships. The movement need be only of the smallest dimension—the conditions of intrusion or eruptivity are as well fulfilled by a movement of a centimetre as by one of a kilometre. If the movement is on a considerable scale the result may be, as Delesse stressed in 1869, that the intrusive body has been detached from its original genetic environment and appears in a new mechanically caused setting. This point cannot be over-emphasized; it must constantly be borne in mind in all discussions of contacts. Consider for example, such contacts as those described and figured by Harker from Skye (1904, pp. 98–9, 132–5, and Figs. 20, 29, 31). In these, gabbro or granite shows extremely sinuous contacts with limestone; the shapes of the limestone remnants undoubtedly indicate (to me, that is) corrosion and replacement by magma; the fact that neither the gabbro nor the granite show any signs of contamination with limestone indicates (to me, again) that such products of interaction between magma and limestone which were once at the contacts have now moved on to some other position. It is an entertaining thought that these undoubtedly remarkable reaction products may present, in their new position, problems perfectly insoluble on the local contact evidence.

With reference to our own special inquiry concerning how to get the products of granitization to become intrusive, I may recall certain statements in previous pages of this present address and also of its forerunner last year. We have seen that Wegmann, for example (Wegmann and Kranck, 1931, p. 58) suggested that the Hangö granite was mainly a rock-mass soaked with granitic solutions; where the products of this soaking remained in the

position of their formation, we have an area of granitization; soaking, however, may give the soaked mass the power of movement and if this occurs with sufficiently resistant walls a granite intrusion may result. We may recall, too, Wegmann's proposals (1935*a*, pp. 334–8) for recrystallization in the highly compressed arches of the infrastructure with the result that granitic stocks break through the folded gneisses. With continued kneading and recrystallization of the infrastructural tongues, all signs of their migmatitic origin may be lost by the resultant granitic intrusions. In the deep zones intrusive rocks reach their places by virtue of three qualities of movement—molecular movement of material, folding movements with the country-rocks, and marginal movements (Wegmann, 1936, p. 45).

If we cast back to my remarks of last year (Read, 1943, pp. 70, 71; this volume, pp. 56-8) we note that the Lake District School of Clifton Ward and A. H. Green considered that increase in the degree of the metamorphism in granitization would lead to intrusion. Ward, for example, states that 'a granitic mass formed by metamorphism of rocks *in situ* is almost sure to become an intrusive mass *at some part*' (Ward, 1876, p. 30). We may remember, too, Green's three forms of granite,—bedded and amorphous granites, produced by metamorphism in place, and irruptive granite, 'forcibly intruded into the rocks among which it occurs'. This 'irruptive behaviour may reasonably be attributed to an increased degree of energy in the metamorphic process which gives rise to it' (Green, 1882, pp. 450–1).

That the products of granitization could move was a tenet of the early French school. Thus, for example, Virlet, in the 1847 volume which we have already examined, is explicit on this point:

'Le surgissement de telle ou telle roche n'entrâinnent pas nécessairement toujours la conséquence d'une origine ignée, parce que le métamorphisme a pu, dans de certains cas, donner lui-même lieu et en agissant par pression sur les masses inférieures, à des surgissements analogues.' (Virlet, 1847*b*, p. 505).

Let us again consult the Fennoskandians on this matter. As we have noted in an earlier page, Sederholm (1926, pp. 134–5) held that 'the ichor of the granite has been able intimately to penetrate the older rocks giving them a new "palingene" eruptivity'.

Backlund, it will be recalled, has erected a series of stages of increasing mobility, with the end terms of migmatite, palingenite and diapirite, which result from the increased activity of ichors. He has considered this question of *mobilization* at some length. Before I deal with his contributions, however, I should state that to me mobilization is an ordinary English word with its ordinary meaning, namely, making movable. It will be a bad day when mention of mobilization 'is a bell to summon the fantastics'. If a technical geological term is required (for, I feel, the purpose of confusing the issue) you may care for *rheomorphism* which is defined by Backlund as:

'the whole process by which a rock is converted partly or entirely into liquid with the entry of more or less new material which has diffused in' (see original, *ante*, p. 133).

Backlund considers that granitization leads to an increase in the amount of material in a given space and that this increase is small in the case of the pelitic host so that the granitization products of this parentage remain in position, but in the case of limestones and quartzites, the increase is considerable so that the products become mobile and may form intrusions. In my opinion, the fundamentals of Backlund's argument are open to doubt; since there are no great changes in the specific gravity of rocks on granitization and since the geometry of the original rocks also is often little changed, I prefer to believe that if immigration takes place, emigration takes place also. The explanation of mobilization is, I feel, not to be sought along these lines.

We may now examine certain British proposals on mobilization. After their excellent exposition of the chemical and physical mechanism of the metasomatism involved in granitization, M. MacGregor and G. Wilson (1939, pp. 210–2) proceed to discuss whether metasomatism alone can produce mobility. They remark that if sufficient intergranular fluid is produced, cohesion between the rock constituents will be lost. A mass composed of spherical grains would become mobile if 26 per cent. of its volume became liquid. In nature, with irregular grains, introduced fluxes and wet conditions, mobility would be more readily attained. Changes of temperature and pressure and the introduction of emanations into

the already heated rocks assist in the development of mobility. It seems to me that proposals along these lines are entirely reasonable; there can be no question that such mashes of crystals and liquid could move on this kind of ball-bearings.

Dr. Doris L. Reynolds has on many recent occasions discussed mobilization; I consider one (1936*b*) as an example. She (loc. cit., p. 355) makes reference to material which 'is *igneous* only in the sense that its material temporarily attained mobility as a local result of ultra-metamorphism stimulated by the introduction of highly energized emanations'. Sediments are considered to become endowed with the ability to move by a process of transfusion which is 'essentially one of metasomatism due to emanations. The actual movement . . . of the transfused sedimentary material is possibly to be attributed to either internal expansion, dependent on metasomatic additions (including energy), or to externally applied stresses' (loc. cit., p. 355). The mobilized sediment 'provides evidence of an arrested stage in the generation of magma' (loc. cit., p. 358) and 'the process of transference culminates in the development of magma' (loc. cit., p. 360). For my part, I think I should prefer to use migma instead of magma in these last statements.

It would be mere diehard obstinacy to deny the possibility of mobility being induced in granitization products by pore-magma, introduced fluids, increased temperatures and increased pressures. No one denies mobility in the phenomenon, mechanically somewhat analogous, of hill-creep and slumping. If, however, movement has occurred in the granitization products so that they have become intrusive bodies, should not evidence of this movement be obtainable in the consolidated intrusive rocks? A mash of crystals, lubricated with a rather small amount of fluid, is forced into a new position; surely movement of material of this composite kind will be recorded in the texture of the final rock? In the opinion of MacGregor and Wilson (1939, p. 210) 'the complete fluidity of any major mass has never been conclusively demonstrated. In most cases a mobile mush of crystals and liquid would equally well account for the observed facts.' This statement, I hold, is a true one. There are a great many textures in the crystalline rocks which can be interpreted in a variety of ways, depending upon the dominant belief of the observer. Further, in the mashes

we have in mind, hot and soaked and communicating with active sources, there will be great opportunities for homogenization. Lastly, undoubted evidence of flow of solids in fluids is available from many intrusive masses.

VIII. GRANITES AND GRANITES

In the preceding section of this address, I have considered what is in essence the production of migma. We must remember, however, that according to many observers magma, *completely fluid rock-substance*, may arise during granitization. We have noted, for example, Sederholm's proposals concerning anatexis by which bodies of magma may be formed. We have noted also, now and last year, other suggestions on granitic magmatization such as those of Hutton, of Lyell, or of Termier which favour an intensification of metamorphic processes, or that of Eskola of selective refusion. All such bodies of magma may be forced into other positions and there consolidate as igneous rocks; they may form some of the puny cross-cutting granites of the higher levels. When in addition we recall also the proposals outlined in my address of last year (Read, 1943, pp. 78–80; this volume, pp. 72–4), concerning the formation of primary granitic magma by the melting or partial melting of the sialic layer, we realize that there may be, in Green's words, granites *and* granites.

It is my opinion, however, that in reality we do not have much freedom of choice amongst the many proposed origins of granite. Our choice is restricted by one gigantic problem, that of room—how do granite masses get into their positions? In the case of small granites we may solve the problem along structural lines, by suggesting that doming, cauldron-subsidence, block-faulting or other mechanisms made possible the entry of granitic magma or migma. For large masses of granite, however, such solutions are impossible —no mechanism yet devised accounts for the intrusion of the gigantic volumes of magma there required. The only solution is to suggest that in fact no large bodies of granitic magma were in existence. The large granite masses result from replacement, they are granitization products. Many of the small granites may of course also be of the same origin, but some of them may result

from the consolidation of migma and some from the consolidation of magma. Thus, though there may be granites and granites, most of them are of one kind and all of them may likely be of one connected origin.

The attentive and pugnacious listener may interject that I am using the term granite in a scandalously loose fashion. Did I not, he may ask, define granite last year as a deep-seated igneous rock, and did I not define an igneous rock as one consolidating from a magma, and a magma as completely fluid rock-substance? I reply that I did so define magma and igneous rock, but I protest that the definition of granite presented was that of the standard authorities and not mine. In my opinion, some few of the granites may be igneous rocks as I have defined them, but there are a great many granitic rocks which are not igneous. The definition of the authorities is excellent for one group of granites, but is not valid for most. I decline to propose a new set of rock names because Authority has taken too narrow a view and has concerned itself with minor presentations of granite. If you take the trouble to turn back to the second page of Part One of these Meditations you will find an excellent definition of granite—excellent, I suspect, because it is provided by a man with little petrological knowledge. It is that given by the *New English Dictionary*: Granite is '*a granular crystalline rock, consisting essentially of quartz, orthoclase-feldspar and mica, much used in building*'. Here are all things necessary—any further limits are but shackles on the free spirit.

IX. RETURN TO LYELL

In our examination of the work of the French School we saw that many prominent members thereof—Virlet, Élie de Beaumont, Delesse, Michel-Lévy, Termier, to name a selection—held that there was a close connexion between granite-formation and regional metamorphism. De Beaumont, for instance, reiterated that the origin of mica-schists, gneisses and granites is one connected question—a vast problem of which the origin of granite is the kernel. Again, Delesse and Termier considered that the formation of magma, intrusion and regional metamorphism were all effects of the same cause—high-energy transformation resulting

in plutonic and metamorphic rocks. For Michel-Lévy, contact metamorphism confounded itself with regional metamorphism in the depths. As we have also noted, the same kind of idea is also favoured by many of the Fennoskandians. In Sederholm's opinion the great quantities of solvents required for large-scale regional metamorphism can only be obtained from subjacent eruptive masses. Modern Fennoskandians, especially Wegmann and Backlund, propose that movement of the migmatite front leads to the generation of granitic rocks and the impression of regional metamorphism on great bodies of crust. The majority of geologists who have worked on migmatite ground see a close connexion between granitization or migmatization and regional metamorphism.

This connexion is a matter which I have discussed at some length (Read 1939 (1940), especially pp. 22–9 of the separate; this volume, pp. 32–43), there is no need to traverse the ground again—I make one quotation:

'Out from the central theatre of granitization there pass waves of metasomatizing solutions, changing in composition and in temperature as they become more distant from the core and promoting thereby the formation of zones of metamorphism about it' (loc. cit., p. 27; this volume, p. 40).

I conclude that migmatization is the prime cause of regional metamorphism.

Very early in these Meditations we considered certain aspects of the teachings of the master Lyell; we may now return to them. Lyell's class of the Primary or Hypogene rocks consisted of two groups:

i. Stratified—such as gneiss, mica-schist, clay-slate.
ii. Unstratified—mainly granite or allied rocks.

The unstratified Primary rocks were called Plutonic. Lyell considered that the plutonic rocks were formed by igneous action at great depths, and he supported this view by arguments whose validity we have already examined (Read, 1943, pp. 68–9; this volume, pp. 53–6). It is necessary to remember, however, that Lyell used plutonic as an attribute of a process as well as of a rock.

Plutonic action is so called by Lyell 'because it appears to have been developed in those regions where plutonic rocks are generated, and under similar circumstances of pressure and depth in the earth' (1838, p. 18). When we look into the products of plutonic action we find some interesting matter. Thus, threads of granite are produced in sedimentary rocks by plutonic action (1835, vol. iv, p. 385) and larger bodies of granitic melt may also be a consequence. This plutonic influence has sometimes been 'on so grand a scale (that) we must not consider that the strata have always assumed their crystallization or altered texture in consequence of the proximity of granite, but rather that granite itself, as well as the altered strata, have derived their crystalline texture from plutonic agency' (1838, p. 19). In his early treatment of the metamorphic rocks, Lyell states:

'The metamorphic theory does not require us to affirm that some contiguous mass of granite has been the altering power; but merely that an action, existing in the interior of the earth at an unknown depth . . . analogous to that exerted near intruding masses of granite, has, in the course of vast and indefinite periods, and when rising perhaps from a vast heated surface, reduced strata thousands of yards thick to a state of semifusion, so that on cooling they have become crystalline, like gneiss. Granite may have been another result of the same action in a higher state of intensity, by which a thorough fusion has been produced; and in this manner the passage from granite into gneiss may be explained.' (1838, p. 251.)

Essentially the same statement appears in the 12th edition of the *Principles* 1875, p. 139, and is worth repeating:

'The transmutation has been effected by the influence of subterranean heat acting under great pressure, and aided by thermal water or steam and other gases permeating the porous rocks, and giving rise to various chemical decompositions and new combinations, the whole of which action has been termed "plutonic", as expressing in one word all the modifying causes brought into play at great depths and under conditions never exemplified at the surface. *To this plutonic action the fusion of granite itself in the bowels of the earth as well as the development of the metamorphic texture in sedimentary strata may be attributed.*'

I have italicized the last sentence of this quotation to emphasize

the Lyellian doctrine that plutonic action gave rise to both metamorphic and granitic rocks. The view expressed above that migmatization and regional metamorphism are connected processes has thus a most honourable ancestry. I am of the opinion that in any scheme of rock-classification, the granitic and metamorphic rocks must be placed close together. This is a return to Lyell, and the combined group thus erected in his Primary or Hypogene group. It is regrettable that both these names are not readily acceptable: hypogene is one of a class of names which rings no bell with me, whilst primary has undesirable historical connotations. We have just seen that Lyell's plutonic action was responsible for both members of his Primary group: accordingly, I suggest that we re-employ plutonic in this sense. My first proposition is: *There is a plutonic class of rocks which comprises the granitic and metamorphic.*

We may now return again to the French School for the source of the basis of our second class of rocks. It will be recalled that Élie de Beaumont clearly realized the existence of two fundamentally different groups of eruptive rocks, the one granitic and plutonic, the other basic, dominantly volcanic but including traps and serpentines. Michel-Lévy emphasized this distinction forty years later in his statement that he could not insist too strongly on the lack of either stratigraphical or petrological relation between the laccolites, the true volcanic chimneys with dykes and sills on the one hand, and the granitic masses on the other. Though the development of these views was a French achievement, it is well to remember as we have already seen that a similar contrast between volcanic rocks and Primary rocks was an early tenet of Lyell though, it is true, the master permitted himself later to swallow the igneous panacea. As I displayed last year (Read, 1943, pp. 84–5; this volume, pp. 83–5), a modern presentation of the fundamental difference between the volcanic and the plutonic rocks is provided by the brilliant synthesis of W. Q. Kennedy (1938, pp. 24–82). The two associations, volcanic and plutonic, erected by Kennedy, represent two distinct and apparently independent expressions of magmatic activity. The *volcanic association* is derived from a universal basaltic magma, and includes not only the effusive rocks but also all intrusions related to the volcanic activity and originating in the same magmatic source;

crystallization-differentiation may be valid in producing the diversity of rocks among this association. As we have already noted above, Lyell accepted the great volcanic class of rocks 'undoubtedly recognized to be the products of fire, from the exact resemblances to those which have been produced in modern times by volcanoes' (Lyell, 1833, vol. III, p. 10). Let us, sustained by Kennedy, return once more to Lyell. My second proposition is: *There is a Volcanic class of rocks.*

Where, now, stands this term igneous? Bearing in mind that igneous means consolidated from a completely fluid melt, the magma, we are now obliged to restrict its use. The igneous rocks are the magmatic rocks, and these are dominantly the Volcanic class of our new grouping. This is another return to early Lyell—'volcanic, the products of igneous action' (Lyell, 1938, pp. 4–5). Some few of my Plutonic rocks are most likely magmatic and therefore igneous, but each example proposed must have careful study before the proposition is accepted. Enough damage has already been done by extending this term igneous too widely and too readily. If a rock can be demonstrated to have consolidated from a magma, it is an igneous rock, if not, not.

The remaining class of rocks proposed by Lyell, and not covered by my present suggestions, is his Aqueous—the sediments. It is a pity that we have no obvious name for them which matches with Volcanic and Plutonic—I propose with little hope of its adoption something along the lines of Neptunic. *The third class of rocks then is the Neptunic.*

We have now returned completely to Lyell. There are three great classes of rocks, Neptunic, Volcanic, Plutonic. There are a few odds-and-ends left over that can be tucked in at leisure. I present the following summary of the position:

THE THREE CLASSES OF ROCKS*

1. *Neptunic.*—The sedimentary rocks, dominantly marine.

2. *Volcanic.*—Effusives and associated intrusives; dominantly basic magmatic, igneous; non-orogenic; comprises the orthodox volcanic

* The frontispiece displaying this classification has been kindly provided by Dr. Gilbert Wilson. For this, and for much kindly criticism, I tender my grateful thanks.

rocks plus related minor intrusions, sills, laccolites, &c., as of gabbro, dolerite, andesite, trachyte, &c.; crystallization-differentiation may run in this class.

3. *Plutonic.*—Of two associated kinds, the metamorphic rocks, and the ultra-metamorphic, migmatitic, metasomatic, granitic rocks; orogenic; comprises the great granitic complexes, the gneisses and schists.

I submit these proposals for your consideration. Possibly the day of the heroic gesture is gone, and it better serves the prudent man to be quiet and canny. You may, however, be enticed by these remarks to permit a reorientation, maybe only a temporary one, of your outlook on rocks. If that happens, I am rewarded, if it does not happen, I acquiesce. As an Englishman, I recognize Sir Walter Raleigh's portrait of us: 'If he cannot please by being himself, he is content not to please, and gives the matter no further thought.' I leave it at that.

These Meditations, which have most pleasantly occupied my leisure for two years, are now before you. Once again I make a plea to the reader interested in the matter to consider both Parts together. Finally, I ask forgiveness of my friends at the Royal School of Mines who must have found me for the last two years a person of one topic of discussion. I regret that there is likely to be yet more to come—for in this matter, as in many others, the wisdom of Jesus the son of Sirach is true:

'When a man thinketh he hath finished, then he is but at the beginning; and when he ceaseth, then shall he be in perplexity.'

IV

GRANITES AND GRANITES

1948

INTRODUCTION

THERE can have been few occasions in the past when the President of the Geological Society of London—the mother of all geological societies—has been able to accept the hospitality of that most splendid of daughters, the Geological Society of America. I come bearing most hearty greetings and good wishes from my Society to yours.

This meeting celebrates, I believe, the Centenary of the Geological Survey of Canada, and I am honoured to be present at the inauguration of a second century of fruitful geological investigation. Our countries are bound together by the cords—gossamer yet stronger than steel—of a common blood, a common creed, and a common history—we spring from the same roots. But we geologists are bound still closer to one another by devotion to our common science—nothing can unloose that tie. As these proceedings continue, we shall find maybe that we differ on a great many aspects of geological interpretation, but this is a sign of vitality and can in no way affect our regard for one another as fellow-seekers after the truth concerning the crust of Mother Earth.

As President of the Geological Society of London, I salute the Geological Society of America and, contemplating your glorious past, I look forward to your yet more glorious future.

* * *

In the foregoing paragraphs I have spoken in my official

presidential capacity; from now on, I speak as a private individual—the personal pronoun will occur, I expect, with astounding frequency. I am not certain in what role I appear before this assembly, whether as a prophet blessed with a new revelation or, more in keeping with Professor Bowen's (1947, p. 264) valuation of me, as an old soak seeking the penitent's bench. Maybe, like most men, I fit best into some intermediate category. This question of the origin of granite is perhaps the most lively of geological topics today—but we should remember that it always has been. About every twenty years or so, the problem has been finally settled, and a sort of uneasy peace has broken out. This indicates to my mind that there is no unique solution of the problem—there are granites and granites. Assuming the character of a trimmer (in the ancient and best sense), I protest that there is no need for an *either-or* attitude. Bigots or, if you like, enthusiasts, on both sides do a deal of harm, and pontiffs, I suggest to Professor Bowen, while capable of a greater number of good deeds, are also capable of a greater number of bad deeds than the village drunk. If we keep our tempers, whilst not pulling our punches, we shall receive great profit and pleasure from these debates. I quote from Hutton, the founder of our science:

'While man has to learn, mankind must have different opinions. It is the prerogative of man to form opinions; these indeed are often, commonly I may say, erroneous; but they are commonly corrected and it is thus that truth in general is made to appear.'

What I propose to do on this occasion is to present the results of my consideration of the granite problem that I have reached during the last half-dozen years. This is not the time or place to describe new work, either mine or my students. I have, as it were, to draw up a trial balance sheet of my personal faith in this matter, leaving it to the future to change the two sides of the account. During those magnificent but episodic war years in London, when one went to earth at dusk, I kept myself geologically fit by pondering on granites and granitization (Read, 1943; this volume, p. 44; 1944; this volume, p. 86; summarized in 1946). Today I summarize these meditations and bring them up to date; in doing so, I must needs repeat a great deal that I have already said in other places, and, as time presses, often in the same words.

THE NATURE OF THE GRANITE PROBLEM

I see the granite problem as essentially one of field geology—it is not primarily one of petrography, mineralogy, physical chemistry, or of any other ancillary discipline. It certainly does not fall within Gignoux's category of *la pétrographie de tiroirs*. Granites are very big things, not hand-specimens. Now every geologist must judge a geological question against the background of his own field experience. This self-evident fact determined the divergences between the French masters like Michel-Lévy and Lacroix on the one hand and the German petrographers like Rosenbusch on the other. A great part of my own field-excursions has come from areas of granites, migmatites, and metamorphic rocks of regional development. If, by viewing granites in a certain way, I find that my field-experience can be interpreted more comfortably, then I propose to adopt that view no matter how uncomfortable it may be to another geologist with a different field-background. This is not a matter of spiritual pride but just plain common sense.

To me, the making of granite, migmatization, and regional metamorphism are all parts of one process—they result from the agency that Lyell a century ago called *plutonic*. In his classic metamorphic theory of the origin of granite, the master placed the granitic and metamorphic rocks in the closest genetic association:

'granite itself, as well as the altered strata, have derived their crystalline texture from plutonic agency' (1838, p. 19).

'The transmutation has been effected by the influence of subterranean heat acting under great pressure, and aided by thermal water or steam and other gases permeating the porous rocks, and giving rise to various chemical decompositions and new combinations, the whole of which action has been termed "plutonic" as expressing in one word all the modifying causes brought into play at great depths and under conditions never exemplified at the surface. To this plutonic action the fusion of granite itself in the bowels of the earth as well as the development of the metamorphic texture in sedimentary strata may be attributed' (1875, p. 139).

Before he was borne down, like the rest of his British and American contemporaries, by the onset of magmatism from Germany, Lyell clearly separated the plutonic from the volcanic rocks, the latter being 'the products of igneous action' (1838, pp. 4–5, 11–13). The existence of two fundamentally opposed groups of eruptive rocks, the one granitic and plutonic, the other basic and dominantly volcanic, had likewise been realized by Élie de Beaumont (1847, pp. 1253, 1254, 1288, 1297), and this distinction was later emphasized by Michel-Lévy and others of the French school. The French saw fundamental differences in the mode of eruption of granites and of other eruptive rocks. A modern presentation of these differences is provided by W. Q. Kennedy (in Kennedy and Anderson, 1938) in his division of igneous bodies into volcanic and plutonic associations, the first association being basic, dominantly effusive, truly magmatic and non-orogenic, the second being granitic and granodioritic, associated with processes of assimilation, and with orogenic movement. Drawing from Lyell and the French masters and fortified by Kennedy, I have recently (1944; this volume, p. 166; 1946) advanced a threefold classification of rocks that suits my field experience best. It is:

Neptunic—the sedimentary rocks, dominantly marine;

Volcanic—the magmatic, igneous, rocks, dominantly effusive and basic;

Plutonic—the metamorphic, migmatic, and granitic rocks.

The granite problem is essentially a plutonic problem—the granitic rocks are tied up at one stage or other of their formation with the rest of the plutonic rocks and only rarely and almost fortuitously are they genetically connected with the volcanic rocks of my classification. The contrast in the geological setting of the Volcanic and the Plutonic rocks is of so fundamental a character that I am unable to accompany Niggli, Bowen, and many others in their nimble leaps horizontally or vertically from one pigeon-hole to any other pigeon-hole in the usual classification of rock specimens. If the classification were truly genetic, such leaps would be justified, but I cannot follow, for example, Niggli (1942, p. 6) in the vertical leap from rhyolite to granite which he performs because of the undeniable existence of lavas of granitic composition, nor Bowen (1947, pp. 263, 264) in a similar leap from andesite to

diorite because vast volumes of andesitic lava undoubtedly existed. Even the giant Lyell (1838, p. 217) took two jumps to do such feats. The volcanic rocks can be interpreted along uniformitarian lines; the plutonic rocks are too deep a matter.

Perhaps it was Edward Suess in his *Das Antlitz der Erde* who persuaded us that the transition from volcanic to plutonic was a reality by his masterly reconstruction of a denudation series from a modern cinder cone to a deep-seated mass of granite. As Raguin (1946, p. 127) has recently recalled, such a transition is nowhere displayed as a whole but is the projection of a theoretical view based upon the interpretation of fragmentary observations. Perfect chemical and petrographical transitions may be demonstrable, but the much more important geological transition has to be argued. These are matters discussed in detail by Raguin (1946, pp. 127–41) and W. Klüpfel (1941). The geological factor involves time, a dimension lacking in the *pétrographie de tiroirs* and in Suess's denudation series. Vulcanism and plutonism closely associated on the ground may be separated by respectable stretches of time; their sequence appears to be irregular, and one does not demand the other; their association may be accidental. A conversation (*Unterhaltung*) between Hans Cloos (1939) of Bonn and A. Rittmann of Basle indicates how international this view of the independence of *Vulkanismus* and *Plutonismus* now is: to Rittmann's remark that these two processes have magmatically nothing to do with one another, Cloos replies *Zugegeben*, and later proceeds to reinforce this opinion from his vast stores of knowledge of granite tectonics and the regional settings of plutonic bodies. Time again enters into the conversation—the granitic and plutonic rocks belong to the orogeny, the basic and volcanic rocks belong to the geosynclinal phase—a view of general application as I suggested long ago (Read, 1927).

I feel much more comfortable, therefore, in seeing the granite problem as a plutonic problem not directly attached to basic rocks of the volcanic association. I have before welcomed this 'freeing the granites from their bondage to a parental basaltic magma' (1943, p. 85; this volume, p. 84)—conduct that has not met with the approval of Bowen (1947, p. 274) who reminds us that since the earth is dominantly basic, the original granitic layer at least must have been derived from basaltic magma. I agree, of

course, but protest that this gigantic operation, like a great many others that went on at that distant date, is not necessarily going on now, nor need it ever have gone on by crystal settling. Here is that dimension of time turning up again—and we should remind ourselves that uniformitarianism cannot be true for all time.

It will be convenient at this stage of my argument to recall other expressions of the volcanic : plutonic duality. From the earliest days of our science, the dominance of granitic rocks among the intrusives and of basaltic rocks among the extrusives has been recognized. Daly (1933, pp. 32–41) and Barth (in Barth, Correns and Eskola, 1939, p. 113) have made quantitative estimates that confirm the early suggestions—granites and basalts are the commonest rocks of the crust, and, for a geologist, the most important rocks are the commonest. These remarkable facts early led to the proposals of Bunsen in 1851 and Durocher in 1857 that two magmas, one acid and one basic, were available in the crust from which all the variety of igneous rocks could be produced, and this two-magma view has been argued along modern lines by F. Loewinson-Lessing (1911). It was the belief of Durocher (1857) that the two magmas had their determined positions in the crust, the lighter viscous acid magma resting on the denser fluid basic magma. From geophysical evidence it is considered that Durocher's liquid shells do not exist as such, but the same evidence suggests that a light granitic sialic layer overlies a dense basaltic simatic layer. The very crust itself appears to have a volcanic : plutonic duality.

Before going further, I must give a few more details concerning the contents of my volcanic and plutonic classes only outlined in the foregoing. The volcanic rocks include the intrusive diabases and gabbros which are considered simply as portions of basaltic magma that have failed to become extrusive. Most of the effusive rocks may be interpreted as produced by crystal fractionation of primary basaltic magma; the formidable question of the position of the voluminous andesites must be left till we have a better synoptic view of them, both in time and space. Turning to the plutonics, the granitic members with which this debate is primarily concerned include, of course, the granites and granodiorites. The unimportant syenites and diorites must each be considered as a special case, and its origin determined on the field-evidence, but

in many examples they appear to me to be of hybrid or contamination origin closely attached to granite. Parenthetically, I express the hope that I shall be able to take back to Britain a chip of the normal igneous diorite known to Professor Bowen (1947, p. 263)—it will be a pleasant memento of this visit.

To summarize this first section of my remarks, then, I present the two classes of rocks—volcanic and plutonic—differing fundamentally in their nature and geological setting. The granitic rocks of this debate are held to be plutonic.

MAGMATIC DISPLACEMENT VERSUS NONMAGMATIC REPLACEMENT

If we have to select a unique solution for the granite problem then the choices are two:

(1) Granites are igneous rocks resulting from the consolidation of an intrusive magma that structurally displaces the country-rocks.

(2) Granites are rocks resulting from a granitization during which the country-rocks have been replaced (or, better, transformed).

It will be an advantage to state the senses in which I use the technical terms. The *New English Dictionary* defines *granite* as 'a granular crystalline rock, consisting essentially of quartz, orthoclase-feldspar and mica, much used in building'. With the extensions requisite to cover granodiorite and the like, and with the added information that can be gathered from a piece of granitic rock—namely, that the constituent minerals are large enough to be recognized by the naked eye and that, under the microscope, they are seen to have interfered with one another's free growth—we have a perfect definition of granite as a rock specimen. The *igneous* rocks are those produced by consolidation of *magma*, which is completely fluid rock substance. If a rock cannot be demonstrated to have been formed by consolidation of magma then it cannot be demonstrated to be an igneous rock—a reasonable requirement often disregarded. Lastly, *granitization* means the process by which solid rocks are converted to rocks of granitic character without passing through a magmatic stage.

THE GRANITIC MAGMA PRIMARY?

If granites are igneous rocks then we have to consider the proposals concerning the origin of granitic magma. These proposals fall into two groups—in the first the granitic magma is primary, in the second it is derived.

We have already noted the classic view of Durocher and Bunsen that there are two independent primordial magmas, one providing the granitic rocks, the other the basaltic. If these two magmas must be made, it appears to me reasonable to derive them from the two outer earth shells postulated from earthquake investigation. As I have said before, two shells, two magmas seems sound. From the sialic layer there might be produced granitic magma by pure melting or by partial melting or solution. Pure melting might be brought about either by depressing the sial into the hot sima or by raising hot simatic material into the sial. E. M. Anderson (in Kennedy and Anderson, 1938, p. 78) sees no difficulty in the suggestion that along the geosynclinal belts the cover may become thick enough to cause the sial to melt and the resulting granitic magma to rise up into the fold roots. Many of the characters of batholiths appear consonant with this suggestion. Pure melting as a means of producing magma has been favoured by many eminent petrologists, as recorded by Daly (1933, pp. 289–90). 'Many granitic magmas may have their immediate origin in the remelting, say by deep burial, of a granite,' states Bowen (1928, p. 319). On the other hand, the association of granitic and basaltic rocks in certain igneous complexes might be explained, as A. Holmes (1931; 1932) has proposed, by hot basaltic magma rising through and melting the overlying sial. I would add here that bodies of granite of this origin would be smallish and of restricted association.

Many geologists who have dealt with regions of metamorphic and migmatic rocks have preferred selective re-fusion to pure melting for the production of granitic magma. In this regard, the early suggestions of Holmquist, Van Hise, Lane, Daly, and others have been especially elaborated by P. Eskola (1932*c*; 1933) who considers that such magma may arise in connexion with orogenic

movements by the squeezing out of the lowest-melting quartzo-felspathic materials, either from basic rocks or from the deep portions of the geosynclinal piles. The granitic magma so formed moves into higher levels of the crust where it may perform metasomatic operations akin to what I call granitization. Eskola believes in crystal fractionation in the production of Cambrian and later granites but sees no evidence for crystal settling in the great granitic terrains of the Archean.

I find it provocative that Daly (1933) too is quite prepared to accept anatexis of this kind for the Archean granite magmas but feels compelled to put forward half a dozen or so alternatives for magmas of later date. I find it so because the Archean supplies the prime examples of my plutonic class of granitic, migmatitic, and metamorphic rocks. Are there granites and granites after all?

Certain authorities consider therefore that a primary granitic magma could arise by melting, pure or partial, of the sialic crust, once it has been separated from the simatic layer. This magma is independent and not derived from a primary basaltic magma. This classic two-magma view that agrees with the distribution of rock types in the crust would be more acceptable to me—if I had to accept either— than the one-magma view, popular today in certain seminaries, that degrades granitic magma to the status of the dregs of the primary basaltic. This opens a discussion that is entitled, especially in this den, to a fresh section of these remarks.

THE GRANITIC MAGMA SECONDARY?

On many geological grounds it is reasonable to admit basaltic magma as primary in its own right. It is the source of my Volcanic class of true and undoubted igneous rocks. As we have seen, vast volumes of basaltic magma have been delivered to the earth's surface or near it. Sinking of early-formed heavy iron-magnesium silicates from such a magma would leave a residual enriched in silica and alkalis, a residual tending toward a granitic magma. By the squeezing out at appropriate stages of the fluid residuum from the early-formed crystals, either through pressure during orogeny or just by the weight of the mesh of crystals, there could be collected partial magmas of various qualities; the extreme term

is held to be granitic magma giving the granitic rocks. I do not propose to attempt in this assembly to discuss the validity of the general theory of crystallization-differentiation as I am sure that that will be well looked after by Professor Bowen; I confine my remarks to certain criticisms concerned with the production of *granitic* magma *in quantity* by this process. These criticisms have already been detailed (Read, 1943, pp. 81–4; this volume, pp. 79–82).

A first criticism is concerned with the nature of the residual liquid arising by the crystal fractionation of basaltic magma. Fenner (1929; 1931)) has argued that such liquids should be rich in iron. Wager and Deer (1939) have provided a field-demonstration on a grand scale that this is the case and conclude that crystal fractionation of basaltic magma leads to ferro-gabbros and not to intermediate rocks of the calc-alkaline series. Niggli (1942, p. 72) dismisses this conclusion as a mere generalization from a special case, and Bowen (1947, p. 273) damns it with faint praise as being only one possible result of the crystal fractionation of basic magma. It seems to me, however, to be a test of the one-magma view on a very noble scale—amounting to a great many 'little crucibles'. The second criticism concerns the quantity of the residuals produced during the fractional crystallization of basaltic magma. This criticism must be considered with the dominance of basalt *and* granite in the crust in mind. Many petrologists—Grout, Holmes, Daly, Krokström, Fenner, and others—have pointed out that only a small quantity of granitic magma can be formed in this way. Daly (1933, pp. 401, 425) has remarked on 'the stupendous quantity of basalt that would have to crystallize in order to make a batholith of granite by [this] process. Is such volume credible in the case of any Post-Archean batholith?' Remembering the granitic cast of the Archean, I propose for one to answer Daly's query, generalized, in the negative. The third major criticism deals with the mechanism of the expulsion of the residual flow from the crystal mesh. Holmes (1936*b*) has dealt with this matter. This operation is of a kind and of a magnitude that it should be capable of ready geological demonstration—and that is certainly not the case.

Bowen (1947, p. 272) has pointed to those geologists who 'claim that the proportion of granitic residual liquid is so small that it could never be squeezed out of the crystal mesh to form a separate

mass' as being the same people who find no difficulty in squeezing out small quantities of interstitial granitic liquid formed by anatexis; this is probably true, but that does not make the residual liquids of Bowen's mechanism any the more abundant. This is, I feel, another example of what Doris Reynolds (1947*a*, p. 212) has recently called authoritarian bluff. Crystal settling can be observed to operate in the laboratory, and it can be proposed to have operated on a gigantic scale in the formation of the simatic and sialic layers of the early earth—there is enough basaltic material for that, at least. But whether it operates or not in the crust is a question that can be decided mainly on field evidence. Petrology is not a restricted branch of physical chemistry—to quote a protest of Reynolds (1947*a*, p. 222), but is a branch of geology. On geological grounds, I maintain the independence of the granitic plutonic rocks and the basaltic volcanic rocks. At my time of life I do not propose, in Hutton's prophetic words, to 'judge of the great operations of the mineral kingdom, from having kindled a fire, and looked into the bottom of a little crucible'.

THE ROOM PROBLEM

It is the problem of room that lies at the heart of the granite problem, and the room problem is a matter to be dealt with by field geology. Let us for the moment concede with the magmatists that granitic magma is available in great quantities in the crust. We have to inquire how that magma, in small and large draughts, becomes emplaced at higher levels. Two types of mechanism have been proposed, the first by structural readjustments of the country-rocks and the second by piecemeal stoping of the cover by the magma.

Small bodies of magma could undoubtedly become emplaced to give igneous masses of sill, laccolith, or dyke form. Nobody can quarrel with that. Even gigantic bodies of magma have been intruded as sheets as, for example, in the Sudbury and Bushveld masses—but these are of basic material. The great granitic masses do not exhibit a sheet form, and igneous bodies of this form are not customary in the fold zones. However, I see no objection to the emplacement of small bodies by doming or other structural

adjustment of allied type. Rather larger bodies may be emplaced by subsidence of a segment of the crust along straight or curved fractures. By such a mechanism, as displayed by Billings (1945), it is considered by Bowen (1947, p. 274) that the room problem is solved, but this seems to me to be beyond the warrant. In New Hampshire, Billings has described a province characterized by ring intrusion controlled by subsidence of blocks bounded by circular fractures and has extended this mechanism to account for the emplacement of the White Mountain batholith of that region by the coalescence of many such ring-dykes. It is important to get the size of ring-dykes in proper focus: the average radius of the New Hampshire examples is 1,900 ft., and the largest in the world, to quote Billings, has a radius of just over 9 miles. A survey of other ring-dykes in Britain and in Nigeria shows that they are never large bodies, and, further, observations on the West African province by a student of mine, Dr. R. R. E. Jacobson, suggest that they are of specialized petrographical types. (So noteworthy is the identity of the Nigerian and New Hampshire rocks and structures that I suggest you have another look for tin in the Eastern States). In any event, ring-intrusions seem to me to be of a small and specialized character—they are not the common rocks we are interested in today. To pass the ball back to Billings and Bowen, I invite them to contemplate the emplacement of the enormous magma reservoirs underlying the ring-structures and shown in many of the diagrams illustrating Billings's work. They seem to me to have lowered the room problem by a few thousand feet, not to have solved it.

Turning to the stoping hypothesis, one would be foolish to deny its efficacy over a limited range. In some of the Cornish granites, for example, its operation can be inferred over tens of feet—the country-rock blocks look as if they could be fitted back into the roof. But this is a roof or margin phenomenon, and it is questionable whether blocks of ordinary country-rocks could sink far in a magma of granitic composition expecially if, as Bowen (1947, p. 274) would have us believe, the large granite masses are merely cappings to still larger basic masses below. Further, as soon as the stoped blocks sank out of the relatively impotent marginal regions, they should become incorporated in the active magma and give rise to derivatives that should be revealed in the

subsequent history of the province. But in many granite masses it appears extremely unlikely that stoping can have occurred. To begin with, the stratigraphy and structure of the country-rock can be traced through the granite; blocks in the granite have not moved. Further, granite tectonics shows that in many plutons there has been upward movement of a viscous material. As Mayo (1941, p. 1069) has remarked for certain Californian masses, 'it is not easy to imagine much downward stoping where highly viscous masses moved upward' and he clinches this valuation of stoping by recording the 'fact that throughout the area, with a few local exceptions, the internal structures of granitic and metamorphic rocks alike are conformable parts of the regional pattern' (p. 1070). Similarly, Ernst Cloos (1936, pp. 436–8) has noted many features of the Sierra Nevada pluton in the Yosemite region that tell against the stoping hypothesis.

Except for small bodies, Mayo rejects the hypothesis of forceful intrusion on account of the disharmony between the enormous force required for such a process and the existence in many cases of relatively thin roofs, and on account of the nature of the wall-rock structures. He falls back upon 'permissive' intrusion, tectonically controlled; it is of interest to me to find that even in such a process Mayo (1941, p. 1074) considers that 'wherever the tension counteracted somewhat the local pressure, the deep-seated rocks are melted, or the travel of heat and fluids was somehow facilitated. As a result, melts, or softened masses of rock might rise and fill any potential cavity as it formed.' But the room problem is still not solved, and Mayo ends his detailed eighty-page paper with a pregnant sentence—'if it can be shown that some of the so-called intrusive rocks were derived from metamorphic rocks, the demand for space for the emplacement of the intrusions will be correspondingly reduced'. In other words, he would be more comfortable about it all, and a similar desire in others has led to the development of the granitization theory, involving little or no magma, that we now proceed to examine.

GRANITIZATION

Granitization means different things to different people—to me

it means what I have said on an earlier page: the process by which solid rocks are converted to rocks of granitic character without passing through a magmatic stage. The Transformists, as Reynolds (1947*a*, p. 209) has happily named them, propose that no great volumes of granitic magma are involved in making the great granitic masses—these are the product of piecemeal transformation on the spot of the country-rocks, and the granites are thus only incidentally igneous. I have given a detailed analysis elsewhere (Read, 1944; this volume, p. 90) of the development of ideas on granitization, but I feel compelled to give also a summary here—since the ideas are largely European.

During the last century, French field geologists have developed views on granitization and felspathization that are now coming into their own. I give my summary of their achievements (Read, 1946, p. 66):

'The French emphasized that there are many kinds of granite contacts; in some the country-rocks are sharply delimited from the granite, in others the country-rocks gradually pass into granite through a transitional zone of metamorphic and felspathized rocks. In this latter type, abundant transfer of material from the granitic zone into the metamorphic zone could be determined. They correlated style of contact with depth of granite formation, clean and definite in the upper levels, hazy in the lower. They related this to the operation in depth of a granitization-process involving either an intensification of metamorphism or the activity of a granitic melt which itself produces the metamorphism. In either operation, they invoked the action of energetic fluids and gases—mineralizers, volatiles or emanations—that made the country-rock as it stood over into granite or prepared it for an advance of the granitic melt. They envisaged conditions under which metamorphic rocks could become igneous rocks.'

For the most part, the French geologists maintained, and still do, that a 'granitic magma was involved in granitization—its intrusion was preceded by a cortège of emanations that transformed the wall-rocks into granite and so made possible their assimilation by the advancing magma'.

During his life work in Finland, the great master Sederholm demonstrated a wide extension of the granitic phenomena made familiar—to anyone who would take the trouble to read about them

—by the French. He proposed a regional re-fusion of deeply buried portions of the crust—a re-fusion connected with granitic magma. This magma mixed with the country-rocks to produce mixed rocks, the migmatites. As his investigations proceeded, Sederholm laid more stress on the activity, metasomatic or otherwise, of granitic juices or ichors in the production of migmatites—but still granitic magma was regarded as their prime source. Under the influence either of ichors or of magma, the country-rocks could be converted to a new anatectic magma that, on consolidation, gave igneous rocks (as I define them).

It is fair to state that neither the classic French school nor Sederholm proposed thorough-going granitization in the modern sense—a granitic magma of one provenance or another was always in evidence; around this igneous rock was a felspathized and granitized aureole. I think it is true that today a respectable but not particularly vocal party would adopt this moderate position —we shall assess the strength of its field-evidence in a moment, but first have to display the developments leading to the pure granitization or nonmagmatic theory advanced by other Fenno-skandians such as Wegmann, Kranck, and Backlund.

Wegmann and Kranck (1931) re-examined some of Sederholm's classic ground in south-west Finland and reached conclusions of great importance for the granitization theory. The celebrated Hangö granite was considered by Wegmann not to be derived from a molten silicate mass but to arise by granitic solutions soaking into the country-rock and rendering portions of the transformed rocks mobile enough to behave as intrusions; the pegmatite dykes were interpreted as parts of the granitizing solutions that crystallized in open spaces in the soaked mass. Around this central core was formed a mantle showing different kinds of enrichment in different parts—internally one of Si-K enrichment leading to the production of quartz and felspar, medially an Mg enrichment revealed by cordierite, and externally an epidote-rich zone of unakites. Since material was brought in at one place it must disappear at another place, and a whole series of chemical readjustments was set in motion, controlled by concentration, permeability, temperature gradient, and deposition. The petrological record of these operations is presented by Kranck. In the granitization zone he finds the typical minerals

to be cordierite and anthophyllite; farther out andalusite, kyanite, and garnet appear, and at the exterior staurolite and tourmaline are formed at the lowest temperatures. The process is one of exchange of minerals rich in MgFe(Ca) against KAlSi—a magnesia metasomatism associated with the formation of a potash granite. Kranck concludes that granitization is a metasomatism rather than a migmatitic intrusion (Kranck, 1937, p. 85).

In his general discussion of migmatization, Wegmann (1935*a*, 1935*c*, 1936) developed the doctrine of fronts, hinted at above, that has now become one of the main features of the granitization proposals. Migmatization passes like a wave through the country-rocks leaving a granitic and gneiss zone behind. Fronts of different qualities move at different speeds with different results through the framework of the diverse country-rocks. Wegmann emphasized the importance of an intergranular film or pore-liquid in this process. The relation between supply and cooling determines the movement of the front; if the front is stationary it may give a sharp contact. As one front advances it may cause the advance of another; for example, the formation of a soda or potash front as witnessed by felspathization may push out the unwanted magnesia and give rise to a cordierite front. A pint pot holds not more than a pint, as I have remarked elsewhere. Wegmann confesses that the source of the emanations—to use a nice noncommittal word—is not known; they must come from the depths in great quantity, concentrated perhaps by crystallization-differentiation and squeezed out. Applications of Wegmann's views to well-exposed ground are given in his Greenland work (Wegmann, 1935*b*; 1938).

Backlund, writing about the same time as Wegmann, advanced very similar proposals which he has since developed (Backlund, 1936; 1937; 1938*a*; 1938*b*; 1938*c*; 1946). As the migmatite front advances, different country-rocks react differently; pelites need only a small influx of soda and silica to change them into granite, but quartzites, limestones and greenstones are converted into granitic and other types by more selective and complex metasomatisms. For Backlund, all the so-called magmatic rocks—except basalts and their differentiates—are the results of granitization, this including such processes as migmatization and anatexis. Evidence includes the preservation of sedimentation and folding structures in the

granites, the regional orientation of xenoliths and their relations to dyke-intrusions. The Finnish granite masses are heterogeneous since heterogeneous sediments have been granitized to give them. Backlund concludes that granitization is the only way in which the room problem can be solved for the emplacement of granite in the folded and compressed Archean.

Viewing these later developments in Fennoskandia, we see that Sederholm's requirement of a magma in migmatization has been largely abandoned. Migmatization and granitization have become almost synonymous operations.

Chemical dissection of the granitization process has been largely the work of Doris L. Reynolds (1943; 1944; 1946; 1947*a*, 1947*b*) in her investigations of the Newry granodiorite in Ireland. In 1943, she described 'replacement' bodies of granodioritic rock rimmed by biotite-enriched hornfels zones and showed that Na, Ca, and Si were introduced into the original hornfels to give trondhjemitic types, whilst Al, Fe, Mg, K, H, Ti, P, and Mn were removed to provide the biotite-rich rims. By a wider investigation, furnished with much chemical and petrographical detail, she (1944) demonstrated that various

'. . . granodiorites have been developed from hornfelsed sedimentary rocks, and that their evolution depended on introduction and precipitation of certain constituents, together with internal migration, fixation and removal of certain constituents. The minimum introductions necessary for granitization were Na, Ca and Si, and the material removed from the system at the end of the process was rich in Al, Fe, Mg, and Ca' (Reynolds, 1944, p. 234).

This outgoing basic material is discovered in a biotite-enriched contact aureole and in basic and ultrabasic roof rocks with an 'igneous-look'—to use Grout's term. The zones of biotite enrichment represent an MgFe front, and the main granodiorites of the complex an NaCa front; in the basic and ultrabasic roof rocks a whole series of fronts, primary and secondary, are recognized, and 'each specific rock now represents a subsidiary zone of maximum concentration of some constituents' (Reynolds, 1944, p. 236).

The FeMg zone of Wegmann, characterized by cordierite, has thus become a real *basic front* considered capable of leading to the

formation of igneous-looking basic and ultrabasic rocks. Dunn (1942) applied the term *diabrochite* to the basic complements of granitization, not wishing to use the term migmatite for these nongranitic products. This basic front is a proposal of far-reaching importance in that it may go to solve another room problem—that of certain basic and ultrabasic bodies occurring on the margins of granitic masses or as multitudes of small masses in migmatitic complexes. In the great Sutherland (Scotland) migmatite region (Read, 1931), for example, some hundreds of very small bosses of basic and ultrabasic material (hybrids of Ach'uaine type) are exposed over an area of a thousand square miles; magmatic emplacement of such bodies over such an area seems to me impossible—but a replacement through a basic front associated with the regional granitization may be found to be workable.

Recently Reynolds (1946) has discussed the chemical changes that take place in various types of rock when they react with granitic 'magma'—gathering her data from a score or more occurrences. She finds that the initial change in rocks of all types includes enrichment in mafic constituents and alkalis and that only subsequently are the rocks granitized in the strict sense of the term. Thus, pelitic rocks in contact with granite 'magma' become molecularly desilicated and attain the composition of syenite or basic or ultrabasic igneous rocks with the culmination of alkalis and/or Ca, Fe, Mg; during the second stage, the desilicated rock receives Si and alkali and loses Al, Ca, Fe, Mg and approaches a granitic composition. A somewhat similar twofold operation is shown to have occurred in quartzites, limestones, and basic igneous rocks subjected to 'granitic' reaction.

It seems to me that the granitization theory, as a result of the development of the notion of basic fronts, is now in a yet more interesting condition than normally. If it is demonstrated that there is a zone of FeMg enrichment around any granite mass, its explanation may be an entertaining matter. In Miss Reynolds's own words (1947*b*, pp. 212–3)—'it has to be assumed either (*a*) that considerable proportions of Fe and Mg remain in the residual solutions of granite magma, which is contrary to experimental evidence; or (*b*) that gaseous or volatile compounds of Fe and Mg are given off from granite magma, i.e., that the process is one of pneumatolysis; or (*c*) that the aureole of Fe and Mg enrichment is

a basic front, to be explained by the fixation of material that migrated from a central locus, now occupied by granite, at the time the granite was emplaced; the granite itself representing, not a crystallization product of magma, but the granitization product of the country-rocks within which it occurs'. There is plenty of work still to be done.

DENTS DE CHEVAL

In the previous section of these remarks I have given what may appear a theoretical armchair interpretation of the granite problem and, accordingly, I hasten to remark that this is essentially a deduction from vast amounts of detailed field-work in well-exposed ground. I join with Miss Reynolds (1947*a*, pp. 209–10) in protesting against suggestions by magmatists like Grout (1941, p. 1565) that transformists 'state that there is evidence but do not give the evidence to their readers'. The main purpose of any paper surely is to give the evidence to the reader.

The field-evidence for granitization comes from all over the world and especially from those regions where the Pleistocene glaciation has laid bare exceptionally good exposures. Scores of sections could be listed that show transitions from country-rock to granitic rock through an intermediate zone of migmatization and felspathization. I have already given details of a picked half-dozen of such investigations selected because they were embellished with many plates and figures (Read, 1944, pp. 81–3; this volume, pp. 147–51)—the list is Barrois's Rostrenon contact, Goldschmidt's Stavanger investigation, G. H. Anderson's work in the Northern Inyo Range, Wegmann's two areas in south and north-east Greenland, Fernando's felspathized schists in the Shetland Islands, mine and Cheng's work on the Sutherland migmatite complex, and Doris Reynolds's study of the Newry transformations to which reference has just been made. The transition country-rock—migmatite—granite is without doubt one of the most firmly established facts. But this transition is interpreted in two ways: *either* it shows a relatively unimportant migmatized and felspathized zone between the country-rock and an intrusive magmatic granite, *or* it shows the production of granite from country-rock by the completion of those granitization

processes that are seen in a half-way stage in the intermediate migmatite zone. We have just seen how Miss Reynolds's elaboration of the basic front may affect our choice of interpretation. It may also be affected by what Raguin (1946, p. 183) has styled '*le phénomène de la double enclave*'—the occurrence of felspars, identical with those of the granite, in the country-rock. I have dealt with this elsewhere at some length (Read, 1944, pp. 74–87; this volume, p. 145).

Such felspars were commented upon by the French a century ago, and the phenomenon became so familiar to them that felspathization seemed a reasonable process. Harker (1932, p. 250) and Niggli (1942, p. 29) have objected that in metamorphism felspars can arise without addition of material; but such rocks are not like the felspathized schists of the migmatite zones (Read, 1944, p. 81; this volume p. 146) as a glance at any of the figures illustrating the selected samples already mentioned would show. Thus, Barrois (1884, p. 14) gives a figure showing irregular veins of porphyritic granite in micaceous greywacke in which are developed numerous large crystals of orthoclase, '*disseminées au hasard, à la façon des andalousites dans un schiste, au voisinage du granite*'. Michel-Lévy (1893–94) made famous similar felspars from the Flamanville granite under the local name of *dents de cheval.* They are recorded from innumerable granite contacts and are displayed with advantage in almost every polished slab of granite used for building. Chemical and mineralogical investigations of many examples show that the large felspars occurring in enclaves and country-rock are identical even in small detail with the large felspars occurring in the 'magmatic' granite. The most remarkable example of 'the phenomenon of the double enclave' is provided by the rapakivi granite of Finland which we may now examine.

The rapakivi granite is characterized by large ovoids of potash felspar mantled by a ring of small oligoclase crystals. An extraordinary variety of explanations has been advanced by magmatists to account for these mantled ovoids (Read, 1944, p. 75; this volume, p. 137).—by crystallization of drops of magma, repeated sinkings and coatings of early crystals, eutectic crystallization of orthoclase and oligoclase, alternate super-saturation for these two felspars, reduction in gas pressure, crystallization in a very

viscous magma, and so forth. As Backlund (1938*c*, p. 367) has remarked, 'no routine explanation based on the usual concepts of "magmatic" behaviour satisfies all the implications disclosed by close examination of the ovoids'. And this remark becomes explosive when it is realized that exactly similar ovoids occur in the country-rock adjacent to the rapakivi. Further, ovoid felspars in granite and country-rock are not peculiar to rapakivi—they have been described for example by Wegmann from Greenland, by Grantham from Shap in the English Lake District, by Fernando from Unst in the Shetlands. I repeat a question I have asked before: are we to believe that the complicated physico-chemical controls required in variety by the magmatists operate also in the country-rock environment in exactly the same way as they did in the consolidating magma?

Some observers have suggested that the large felspars were mechanically forced into the country-rocks; examination of the field-exposures shows that this is impossible—for example, in the Skaw granite of Unst enclaves with felspars can be seen in three dimensions with their delicate sedimentary structures perfectly undisturbed (Read, 1942). Sederholm relies on permeation of the country-rock by the granitic magma. Raguin (1946, p. 184) has argued that the physical states of the aureole rocks and the granitic magma at the time of formation of the identical felspars were the same—the aureole rocks being permeated with solutions and the magma having innumerable particles in suspension. Fenner (1933, p. 81) has suggested that impregnation of the aureole by gases gives rise simultaneously to the same minerals that are being formed from the liquid. Recently, Walker and Mathias (1946) for the famous Sea Point contact at Cape Town have regarded both the porphyroblastic and porphyritic felspars as potash replacements. It must be recalled that the *dents de cheval* are only the most noticeable of the granitic minerals growing in the aureole rocks—other alkali felspars and appropriate ferromagnesian minerals likewise occur, and there is no doubt that thorough granitic rocks are produced from country-rocks by this process. If the country-rock environment and the granitic magmatic environment are considered to be physico-chemically identical then what has been deduced from the 'little crucibles' seems to me to be without application. I would prefer to consider that because the

enclaves containing the felspars are sedimentary, then what encloses the enclaves is also sedimentary—the porphyritic granites have been produced from sediments by an intensification of the felspathization process.

The *double enclave* has been used with great vigour by Perrin and Roubault (1937; 1939) in their objection to the formation of granites from magma. They conclude that granites are made in the solid as the result of large-scale chemical diffusion—a mechanism that we now briefly consider.

DIFFUSION IN THE SOLID

Among transformists, opinion appears to be moving away from the view that granitization is connected with some kind of soaking by juice or ichor and toward the invocation of ionic migration. A dozen years ago, Wegmann discussed atomic migration through stationary and moving frameworks, and Backlund correlated the details of the rise of the migmatite front with the differing atomic radii of the participating elements. Two more recent statements are: granitization is 'a migration of ions within solids by way of structural faults, deformations and crystal discontinuities and by means of potential differences of lattice energies, the result being a re-modelling and substitution' (Backlund, 1946, p. 114); and, 'It is, therefore, necessary to stress the fact that granitization depends, not on the bodily introduction of magma, juice or ichor, but on a complex series of ionic migrations, with balanced additions and subtractions, so that as the rocks undergoing granitization approach the composition of granite, others receive additions including Fe and Mg, and become more basic' (Reynolds, 1947*a*, pp. 210–1).

The whole question of diffusion in the solid state and its bearing on the granitization problem has been considered in detail by Bugge (1946) and by Perrin and Roubault (1937; 1939). The latter deal with a mass of metallurgical and petrographical material of great variety and conclude that granite is formed by reactions in the solid state; they are certain of this since they have not met '*aucun fait qui prouvât, dans le cas d'un granite quelconque, indiscutablement son origine par refroidissement d'un magma fondu*'

(1939, p. 147). I have commented on enthusiasts in an early page and shall do so again immediately. Bugge likewise considers that granites and granodiorites are due to metasomatism in connexion with long-distance migration; 'the rocks are, so to speak, immersed in a molecular and ionic disperse system of particles which moves through the interstices of the minerals and through the minerals themselves, altering metasomatically every part of the rocks' (1946, p. 58). Reynolds (1947*a*, pp. 220–2) has summarized the ways in which solid diffusion can take place—through spaces in the crystal lattice, from one lattice point to another within the crystal mesh, and along the boundaries of closely packed crystal grains. We have to begin to look at minerals not simply as chemical compounds but as chemical compounds with architectures, as J. de Lapparent (1941) has put it.

These new methods of attack on the problem of metasomatism of country-rocks to produce 'igneous looking' rocks appear to me to be of the greatest promise. It is premature to treat them flippantly as Bowen (1947) has recently done. On the other side, it will be of no advantage to the transformists' cause to go too fast. For example, I wish to suspend judgement on some applications of the solid diffusion thesis: I have yet to be convinced, as Perrin and Roubault (1941; 1945) are convinced, that sections hitherto considered to show Mesozoic sandstones and conglomerates resting unconformably on metamorphosed older rocks are really to be interpreted as revealing the metamorphic front which, propagated by solid diffusion, has been stopped on meeting porous layers. I will wait and see; in your idiom, I come from Missouri.

THE PLUTONIC ROCKS

Early in these lengthening remarks, I stated that I saw the granite problem as one closely tied up with migmatization and metamorphism—as a plutonic problem. I have discussed this matter at length (Read, 1940; this volume, p. 1). In many areas of regional metamorphism an orderly sequence of metamorphic grades can be established culminating in a central core of migmatization. I have interpreted this by an application of the doctrine of fronts.

'The process of granitization taking place in the kernel leads to the expulsion of waves of metasomatism which, passing through the surrounding country-rocks under changing chemical and physical controls, promote the formation of the series of metamorphic zones about it. Regional metamorphism, therefore, has little to do with stress or folding or deformation—it is largely an affair of permeation by metasomatizing solutions. It is only by this kind of operation that the delicate original textures of the sediments can be preserved when the rocks are thoroughly recrystallized' (Read, 1946, p. 668).

A great deal of detailed work is to be done in tracing out the movements of the fronts: in Antrim and south-west Scotland Miss Reynolds (1942) has begun by establishing a KFeMgAl front, represented by biotite-rich schists, which moved ahead of an NaSi front, represented by albite-schists—both fronts moving in the direction of axial planes of recumbent folds as advanced guards of a migmatization front.

The proposition that zones of regional metamorphism are related to granitization cores may thus receive support when enough chemical data are available. A first and necessarily generalized survey by Lapadu-Hargues (1945) indicates that the distribution of elements in the series, phyllites: chlorite-sericite schists: biotite-muscovite schists: two-mica gneisses and felspathized schists: granite gneisses: granites, varies with the grade of metamorphism. He interprets the variations in terms of atomic mobility, the smallest ions having migrated farthest; the order of increasing mobility is K, Ca, Na, Mg, Fe. I am of the opinion that the results of Lapadu-Hargues need to be tested statistically and very expanded before they can be accepted, but they show a path that has to be followed.

The close genetic connexion between granitization and regional metamorphism has, of course, long been the belief of many workers, especially the French and Fennoṣkandians, on the plutonic terrains. I have to recall, however, that some modern French geologists, notably Jung and Roques, consider that the migmatite front moves independently of the metamorphic zones (or metamorphic front)—but this is a matter that I propose to deal with in another place. For myself, I find comfort in my view of the plutonic rocks; regional metamorphism seems inexplicable when it is detached from granitization.

MOBILIZATION

Some granites appear to be intrusive, just as some rock-salt, clay, and other mobile materials are intrusive. I interject that, obviously, intrusive does not imply igneous or magmatic. We have to inquire how it is proposed to get the products of granitization to become intrusive—how they are mobilized, to use the current term. We have left chemical mobility and have to consider mechanical mobility.

Early it was realized in France and in England that increase in the degree of metamorphism, ultrametamorphism or granitization would render the products capable of movement. Later, Wegmann and Backlund in Fennoskandia, and MacGregor and Wilson (1939) and Reynolds in England have discussed the mechanism. Mobility could be induced in granitization products by pore magma, introduced fluids, increased temperatures, and increased pressures. A mash of mixed material—Reinhard's *migma*—would move with less than a quarter of its volume liquid. I hold with MacGregor and Wilson (1939, p. 210) that 'the complete fluidity of any major mass (of granite) has never been conclusively demonstrated. In most cases a mobile mash of crystals and liquid would equally well account for the observed facts.' There are a great many textures in the crystalline rocks that can be interpreted in a variety of ways, and in any case, having in mind the nature of the mobile mashes, there will be plenty of opportunities for homogenization.

As a pendant to a great deal of the foregoing, I must remind you that one of the best examples of mobilization is that described by Bowen (1910) from Gowganda, Ontario, where granophyre and aplite veins arise by transfusion of shale at a diabase contact: *Forsan et haec olim juvabit meminisse*, or, to translate, 'those were the days!'

GRANITES AND GRANITES

Whether as migma or, if the process goes far enough, as magma, granitic material may move into higher levels of the crust and produce there the discordant granite bodies with aureoles of

thermal metamorphism. There might arise a whole series of types of granite masses depending upon the mobility of the granitic material—there might be granites and granites. This *Granite Series* has always been a tenet of the French geologists—from Élie de Beaumont a century ago through Delesse, Michel-Lévy, Lacroix, to Termier and the moderns. Termier maintained that if a granite is surrounded by a narrow aureole it is certain that the granite has come from somewhere else, ready made; if a granite is surrounded by a vast aureole of metamorphic rocks, it has certainly been formed in place whilst the neighbouring rocks were regionally metamorphosed. And coming down to last year, we find Raguin continuing that tradition in his *Géologie du granite* with the two categories *granites d'anatexie* and *granites en massifs circonscrits* with transitional types between.

Though there may be granites and granites, most of them are of one kind, and all of them may likely be of one connected origin. Their geological setting will decide where they come in the series. I see vast and inexhaustible fields for steady study and speculation amongst my Plutonic rocks. Let us address ourselves to the task with a good heart.

V

A COMMENTARY ON PLACE IN PLUTONISM

1948

I. INTRODUCTION

THE privileges of the President of this Society are, so far as I see them, doubtful and few. I value most that common to all presidential offices—the permission to speak at large and in the general. I propose today to pay no heed to that counsel which reminds the speaker that half his audience knows as much as he does and the other half considerably more. The reason for this boldness is that I am not to be concerned primarily with some body of factual knowledge accessible to all, but with a personal topic of much greater interest to me—namely, the continued observation of my own geological evolution. In a series of presidential addresses (1937*a*, 1940, 1943, 1944; this volume, pp. 1, 44, 86) in other places, I have meditated, often at length, on current beliefs—both mine and everyone else's— about many fundamental aspects of metamorphism. These exercises have at each stage of them given me a little more comfort in my geological setting; there may not be time enough left for me to attain perfect ease, but I hope still to be able to remove a few of the greater asperities that afflict me.

One other presidential advantage, though perhaps in these days a transitory one, is that shared by Humpty Dumpty—'when I use a word it means just what I choose it to mean—neither more nor less'. When I use the word 'plutonic' it means here what I make it mean, but that is, I trust, just what Lyell made it mean. I propose to examine certain aspects of time and place in the operations of

mainly deep-seated processes in the transformation of rocks. This year I am to be concerned largely with problems of place, next year of time, but, from the nature of the study, no rigid separation is possible. Neither now nor a year hence do I feel any compulsion to forsake my own field. I have no mission to make vapid pronouncements on the world situation nor, wise after the event, to disclose how a proper use of our science would have prevented all our ills. Least of all do I propose to show how this ancient Society should be immediately reformed and properly planned. Today, I shall be myself.

Plutonism comprehends all those operations that give rise to the plutonic rocks, these, as I define them (1944, pp. 91–3; this volume pp. 162–5), comprising the vast transitional assemblage of the metamorphic, migmatitic and granitic rocks. We know what the rock granite is—however much we may dispute the manner of formation of any particular granite; it is a 'granular crystalline rock, consisting essentially of quartz, orthoclase feldspar and mica', to quote the *New English Dictionary*. Metamorphic rocks are not so easily dealt with because, as Daly (1917, pp. 376–91) has so adequately displayed, genetic suppositions creep in and the rock-class that revealed itself as one and indivisible to Van Hise (1898, p. 270) becomes distorted and dismembered to fit a dozen different compartments. Posterity has with a strangely united hand cut off from Van Hise's metamorphism half its processes—those of weathering—and, recently, what was left has been halved again; in Van Hise's own country, Barth (Balk and Barth, 1936) has proposed that the metamorphic rocks should be restricted to those that are recrystallized without change of bulk-composition—a restriction that may logically reduce this renowned group to nothing at all. But the rocks remain, however we change their labels. For me, the metamorphic rocks are those resulting from the operation that Daly (1917, p. 391) has defined as

> '. . . the sum of the processes which, working below the shell of weathering, cause the recrystallization of the original crystalline materials in rocks (with or without chemical reactions) or the crystallization of original amorphous materials in rocks, the change in each case not

being accompanied by general melting of the rock or by general simultaneous solution of its constituents'.

We need not follow Daly, or anyone else, into the debatable subdivisions of metamorphism but can accept this general definition. It is to be noted that pure cataclastic modification of rocks finds no place in it and rightly so, as I have emphasized before (1940). The mechanical breaking-down of rocks along adventitious dislocations is not a process of plutonism and must be excluded from metamorphism. The customary separation of thermal (or contact) metamorphism from regional metamorphism is mainly a matter of academic convenience. So far as the granite bodies are concerned, this separation obscures the fundamental unity of the plutonic processes of granite production and emplacement—at whatever level—and the rock transformations appropriate to the setting. There remains to be noted the third and central member of the plutonic series—the migmatites; I have already discussed their nature and formation at length (1943, pp. 73–5; this volume, pp. 65–7; 1944, pp. 61–73; this volume, pp. 113–30) and content myself by saying that, whatever the origin of the granitic rock concerned may be, the migmatites are mixtures of such rock and metamorphic rock. The plutonic series of rocks is metamorphic, migmatitic, granitic.

The validity of the plutonic series as an independent genetic unit may of course be denied, and especially by those who regard the igneous rocks as an inviolate self-evident and self-consistent class. The significance of the constant association of granitic rocks of some kind and metamorphic rocks of some kind is among what concerns us. For the purposes of this discussion let the association be assumed to be of genetic meaning till, may be, the argument drawn from it either collapses or convinces.

The plutonic geologist, like most geologists, becomes obsessed with space, with the two-dimensional space of map and section and the three-dimensional space of his vision of his ground. But another dimension must come in and that is of time. The plutonic rocks are not mere museum pieces—they would be cinema films if we had the right projector, for, as Sederholm said somewhere, they are not made in one casting. In them is recorded the interplay of rocks and minerals and even atoms, some now quick and some

now dead. They register the happenings in time at a place, and the study of such happenings is geology. The plutonic geologist cannot remain a mere petrographer—he has now to make his rocks come alive.

I intend here, therefore, to conduct a general examination of proposals concerning place and time in the making of the plutonic rocks. I have to deal with such notions as zones, levels, fronts, facies and assemblages, static and kinetic, and all the modern apparatus of metamorphism and rock transformation. So soon as the rocks come alive it is difficult to avoid becoming anthropomorphic—but I err in the best of company. In spite of what I have just said, it is fitting to begin this inquiry with the space-variations encountered by the field-geologist, as it is this geometry that has to be related to time. Unfortunately, it is often obscure as to what is being represented on the geological maps of plutonic regions, since metamorphic variations have been superposed on variations inaugurated when the rocks later affected were first made. This contest between stratigraphic and petrographic mapping is well displayed in the studies of the Scottish Highlands and is a contest that has to be resolved. For our purpose today, the space variations of metamorphic significance are referable to two concepts—*zones* and *facies*—that are most likely confluent.

II. ZONES

Since the time of Hutton and Werner there has been an implicit recognition of a zonal arrangement of the rocks I call plutonic. According to Hutton, the metamorphic rocks are formed from sediments through the activity of the central fire and of invading granitic melts—an activity increasing in intensity with crustal depth. The Primitive Rocks, the deepest of the universal formations of Werner, had granite as their basal parts and gneiss as their upper, and were succeeded by the lowly metamorphosed members of the Transitional Strata. It was the ancient and well-established division of the Central European Urgebirge into an upward succession of gneiss, schist and phyllite that influenced Grubenmann (1910, p. 138) in his formulation of three depth-zones of

metamorphism and, as we see later, modern French workers now interpret this classic succession as one of full stratigraphical import. Lyell's metamorphic rocks were those that 'had been modified by plutonic agency under pressure in the depths of the earth' and accordingly 'they must lie at the bottom of each series of superimposed strata, because the influence of the volcanic heat proceeds from below upwards'. The association of metamorphism with depth colours geological thought throughout the nineteenth century, though the association was determined more by theoretical comparison of different areas than by direct observation of the relations mapped in a single region. Thus Sederholm (1891), who is usually put forward as the originator of metamorphic depth-zoning, simply contrasted the uniform, vertical and coarsely crystalline Archean of South-West Finland with the non-uniform, zonable and lowly metamorphosed phyllites of the Taunus and the Harz, and interpreted this contrast as due to different depths of formation. He did not observe the German rocks to overlie the Finnish and his interpretation is a consequence of the classic doctrines propounded a century before. Similarly, Van Hise (1898, 1904) gave a general deduction from a multitude of different studies when he erected an upper zone of fracture below which lay an indefinite zone of combined fracture and flowage passing down into the zone of flowage.

Van Hise (1904, p. 43) insisted on depth as the dominant geological factor in metamorphism and we must now consider the more precise formulations of depth-zones advanced by Becke, Grubenmann and Niggli. In his famous paper at the International Congress of 1903, Becke (1904, 1913) showed that metamorphic rocks express in their mineral composition and texture definite processes related to pressure and temperature controls. The original constituents, free to react with one another, attempt to reach equilibrium—the reactions being controlled in a general way by the celebrated Volume Law. In the deeper levels of the crust, the reacting minerals strive to make combinations taking up the smallest room. It is the standard textbook practice to give a careful selection of equations that are held to demonstrate the Volume Law—but this is merely an arithmetical exercise. Becke himself seems to me to have demolished his belief in its validity by calling on temperature as the dominant factor in the depths. In the deeper

zone, the temperature is too high for the formation of hydroxyl minerals, in the shallower zone such minerals are possible.

The depth-zones of Becke were elaborated by Grubenmann (1910), who based a complete classification of the crystalline schists upon them. Grubenmann insists throughout on depth-control. Each mineral possesses a *critical level* which it cannot abandon without becoming liable to change. Pressure and temperature conditions vary with depth so that the happenings in each level have a characteristic stamp, and the resulting crystalline schists become bounded in zones. Grubenmann traces the mineralogical and textural changes of a variety of rocks as they become moved from one level to another higher or lower—this proceeding is, of course, an academic artifice to display the zonal classification to the student. The depth-significance of the Grubenmann zones was in some measure diluted in the extension by Grubenmann and Niggli (1924) of the zonal classification to include the products of contact metamorphism and migmatization. Niggli's division of metamorphism into that of the *Grundgebirge*

TABLE A

BECKE–GRUBENMANN–NIGGLI: METAMORPHIC ZONES

Epizone:	Moderate temperature, small hydrostatic pressure, strong stress. Mechanical metamorphism. Chlorite, albite, sericite, epidote, antigorite. Phyllites.
Mesozone:	Higher temperature, higher hydrostatic pressure, strong stress. Chemical metamorphism. Kyanite, staurolite, almandine, micas, hornblende. Schists.
Katazone:	Very high temperature, very strong hydrostatic pressure, weak stress. Chemical metamorphism. Pyroxene, olivine, felspars, micas, sillimanite, cordierite, spinel, garnet. Gneisses.

and that of the shallower and more local *Deckgebirge* has also a certain depth element, though Niggli (ibid., preface, p. viii) explains that this division has only a didactic value.

For the non-specialist, I set out the chief features of the Becke–Grubenmann–Niggli zones in Table A.

The classic French school of plutonic geologists whose results I have summarized elsewhere (Read, 1944, pp. 47–61; this volume, pp. 90–113) likewise held to an increase of metamorphic and general plutonic activity with depth. Michel-Lévy and others of the school elaborated a depth-variation in the nature of granitization and granite-contacts; contact metamorphism in the higher levels passed into regional metamorphism in the deeper. Though he could not bring himself to accept this extraordinary widening of contact metamorphism, Termier (1904, 1912), the perfect plutonist, agreed that regional metamorphism as exhibited in the formation of gneisses was concerned with depth—the *colonnes filtrantes* responsible came from below and chased the old materials in front of them. The latest, and in some ways the most remarkable, of the French work has followed the lead given by Michel-Lévy (1879, 1887*a*) and, taking the classic metamorphic succession of Central Europe—that of gneiss, schist and phyllite—has converted it into a stratigraphical succession, a proceeding that deserves the closest scrutiny.

This modern French work, due chiefly to J. Jung (1928, 1936, 1938) and M. Roques (1941), may for our present purpose be considered in two portions—that being dealt with now concerning metamorphic zones in the usual sense. From their studies in the Vosges and the Massif Central, Jung and Roques propose a separation of the metamorphic rocks into two great groups—*ectinites* and *migmatites*. The ectinites are those crystalline schists whose metamorphism is due to mineralogical readjustment under the control of stress and of circulating solutions; there has been no visible accession of felspathic material. The ectinites form thick and widespread series, variable in crystallization and not closely associated with granites. The migmatites, on the other hand, are the result of a metamorphism accompanied by partial fusion and an influx, usually intense, of alkaline felspathic substances. They are of uniform type, with definite limits, and are abundantly mixed with granitic materials. This classification

appears to me to be somewhat arbitrary and I discuss its validity on a later page.

The ectinites occur in superposed zones representing the successive states of mineralogical equilibrium under the influence of temperature and pressure controls that vary with depth. This is, of course, in the line of the Becke–Grubenmann depth-zones, but Jung and Roques consider that their method of zoning is superior to that of Grubenmann. To begin with, the Grubenmann zones are too coarse and in them are grouped much too thick series of rocks; they do not represent three individualized stratigraphical levels; their limits, varying for different isochemical groups, become indefinite and impossible to place upon the field-map—epizonal serpentine, for example, occurs in mesozonal gneiss. Jung and Roques propose their zones as of more practical use for the field-geologist and erect them on the least number of

TABLE B

ZONES OF ISOMETAMORPHISM OF ECTINITES IN THE MASSIF CENTRAL

(Roques, 1941)

Characteristic Minerals		*Zones of Isometamorphism*	*Thickness*
			Non-metamorphic cover ⩾4,000 m.
Quartzophyllitic or Mica-schist Zone	with chlorite and muscovite	Zone of Mica-schistes supérieurs	3,000 m.
	with biotite and muscovite	Zone of Mica-schistes inférieurs	3,000 m.
Felspathic or Gneiss Zone	with biotite and muscovite	Zone of Gneiss supérieurs	4,000 m.
	with biotite only	Zone of Gneiss inférieurs	⩾6,000 m.

minerals characteristic of the greatest number of isochemical groups. The Jung-Roques zones, established in the Vosges, are valid in the great expanses of the Massif Central—they can be placed on the map in the field. The zones are for the metamorphic rocks what strata are for the sediments. Their essentials are displayed in Table B; the thicknesses given are those proposed by Roques (1941, p. 369) for the Massif Central.

In addition to this stratigraphy of the ectinites, the modern French workers propose a stratigraphy of the migmatites. There is considered to be a certain independence in time and in place of regional metamorphic zones and migmatite belts. There is envisaged a progressive rise of what we now call the migmatite front into higher and higher ectinite zones as we pass across France. These are propositions of fundamental importance in plutonism and I examine them later.

TABLE C

BARROW–TILLEY ZONES

Barrow	*Tilley*
Clastic mica	
Digested clastic mica	Chlorite
Biotite	Biotite
Garnet	Almandine
Staurolite	Staurolite
Kyanite	Kyanite
Sillimanite	Sillimanite

It is fully time that I brought into this commentary the remarkable contributions of British geologists to the study of metamorphic zones. Between 1892 and 1913, George Barrow (see especially 1893, 1896, 1898, 1912*b*) developed the relations between metamorphism and granite intrusion as displayed in the Grampian Highlands of Angus and Kincardine and decided the style of British thought on metamorphic zoning. He established seven zones, shown in Table C, each marked by the appearance of a certain index mineral in rocks of pelitic composition. This pro-

gressive zonal series starts with slates affected by dislocation-metamorphism and finishes with coarse-grained sillimanite-gneisses associated with what we now call a migmatite complex. Barrow regarded the whole series as in the nature of a gigantic contact-aureole around central intrusions of his Older Granite and he considered it 'reasonable to attribute both the minerals and the crystallization to the thermometamorphism of the intrusion' (1893, p. 337).

The Barrovian zones as developed in the pelitic rocks of the Grampian Highlands have been studied especially by C. E. Tilley (1924, 1925; Elles and Tilley, 1930), who has adjusted the zonal sequence as shown in Table C. Tilley (1924, pp. 168–9) introduced the term *isograd* to mean a line joining points at which metamorphism proceeded at similar values of pressure and temperature. Isograd surfaces bound the zones, and rocks are isograde that originated under closely similar physical conditions. The metamorphic correlation (Read 1937*b*) of other rock types with the pelitic zones has been examined by Coles Phillips, Wiseman, Hutchison and others, so that the zonal knowledge of the Grampian Highlands is more elaborate than that of any other equivalent area. The variety of the products is displayed in Harker's *Metamorphism* (1st edition 1932, 2nd edition 1939).

So far as I can recall, any proposal by Barrow of a requisite depth-sequence of his zones was by implication; possibly, his position can be illustrated by his general statement—'the occurrence of sillimanite and coarsely crystalline gneisses over large areas may be easily explained by the great depth at which the metamorphism took place' (1893, p. 352). Harker (1919*b*), in his discourse on matters similar to those we are examining, considered that temperature, the controlling factor in metamorphism, varied 'in a manner having no connexion with depth' and added that 'the same may be premised of shearing stress' (1919*b*, pp. lxxviii–lxxix). But Harker's opinion had become much less definite by 1932, as he then considered that in the Highlands 'advancing metamorphism corresponds with increasing depth of cover' (1932, p. 185) and that 'a zone of higher-grade metamorphism normally underlies one of lower grade, but this relative position may possibly become inverted by subsequent disturbance' (1932, p. 209). Similar views are held by Tilley and Elles (1925, 1930); Tilley

has mapped the garnet zone as lying flatly above the biotite zone in Perthshire, and has postulated that the 'deposition of the zones was [originally] in approximate accord with the depth of burial of the sediments and that movement has been prolonged beyond the metamorphic period, leading to inversion of the zones' (1925, p. 109). Elles and Tilley (1930) hold that the metamorphic zones of the south-west Highlands are recumbently folded—the production of the zones and the folding taking place at approximately the same time. The time-aspect of these proposals is so important that its consideration can best be left for next year. Before we leave the Highlands, however, we have to note that E. B. Bailey (1923) likewise has a succession of metamorphic zones 'superimposed upon one another in order of increasing crystallization downwards'; Bailey's zones are different from the usual Barrovian—rocks with inconspicuous mica form the uppermost zone, below coming rocks with conspicuous mica, followed in turn by a zone of two-fold type, characterized either by mica and albite or by mica and garnet, depending on a wet or a dry style of metamorphism.

We may now look into a selection of extensions and modifications of the Barrovian type of zoning that have been made in other countries. We begin with Scandinavia. In Part III (1915) of his famous researches (1912–21) in the Hochgebirge of southern Norway, V. M. Goldschmidt classified the regionally metamorphosed rocks into

(*a*) chlorite zone with quartz-muscovite-chlorite-schists;
(*b*) biotite zone with quartz-muscovite-biotite-schists;
(*c*) garnet zone with quartz-muscovite-biotite-garnet-schists.

Goldschmidt's garnet zone included higher-grade zones characterized by sillimanite, andalusite, kyanite and staurolite that were not separated out on his zonal map. Many of the metamorphic rocks of the Trondhjem district have a noticeable percentage of calcite in their lower grades and in the highest grade such rocks are represented by calc-silicate gneisses and calc-silicate mica-schists which carry abundant plagioclase with over 10 per cent. of the anorthite molecule; these high-grade types have sometimes been taken to be an oligoclase zone—a proceeding that ignores their special chemical composition. Goldschmidt, of course, realized this and provided three isophysical groups of

varying chemical composition, all characterizing the highest stage of regional metamorphism—these three groups are the garnet-mica-schists, calc-silicate mica-schists and calc-silicate gneisses. We have here an expression of the facies concept, later developed by Eskola, that we examine immediately. Thorolf Vogt (1927) described a comparable series of zones in the Sulitelma Mountains and gave a map on which are delineated the zones of chlorite, biotite, almandine and *oligoclase*. We may examine this *oligoclase zone* more closely: typical rocks in it are oligoclase-muscovite-schists, almandine-oligoclase-mica-schists, and rare kyanite- and staurolite-bearing schists, together with types richer in lime that have abundant clinozoisite and hornblende. There is a close resemblance to Goldschmidt's zones of southern Norway. Vogt describes an increase in metamorphic grade *upwards*, an arrangement due not to inversion of original *Tiefenstufen* but, Vogt suggests, to the 'very common occurrence of granite intrusions in these levels of the mountain range, while such intrusions are relatively rare at lower levels' (1927, p. 483).

The original Barrovian zones were established in rocks of pelitic composition. We have noted Goldschmidt's treatment in Norway of calcite-richer types, and I now consider developments on the other side of the world in the zoning of another chemical type, the greywackes. In New Zealand, F. J. Turner (1930–42) in a lengthy series of important communications has dealt comprehensively with the zoning of the metamorphic rocks of the southern part of the South Island. In rocks of greywacke composition, he has established the zones of chlorite, biotite and oligoclase; this last zone is characterized by the presence of a plagioclase with more than 10 per cent. of anorthite and is correlated with the almandine and kyanite zones of the pelitic Barrovian sequence. Turner's chlorite zone for greywackes is divided into four sub-zones determined by the gradual disappearance of original clastic textures and the gradual development of schistosity and eventually foliation in the reconstituted rock (Hutton and Turner 1936). He shows that rocks of the chlorite zone of greywackes are of higher grade than those of the chlorite zone of pelites. We begin to realize some of the complications revealed by close zoning of the metamorphic rocks; we note others.

As another variant, we may examine T. F. W. Barth's work

(Balk and Barth, 1936) on the metamorphism of Lower Palaeozoic rocks in Dutchess County, south-eastern New York. Three zones are there distinguished: the lowest in grade is that of *muscovite-slate*—slates and phyllites in which the original bedding is still visible; the next, beginning at the biotite isograd, is that of *kyanite-schist*, characterized by schists with biotite, garnet, kyanite and staurolite; and the third is that of *sillimanite-gneiss*, mapped on the entry of sillimanite, and comprising coarse biotite-plagioclase-gneisses and similar rocks in which pegmatites are abundant. There appears to be no depth significance in the distribution of the zones, but rather a relationship of grade to degree of metasomatism, hydrothermal and more purely magmatic. 'By further thorough injection of the augen gneiss with pegmatitic material, the gneiss finally grades into a granite-like rock' (1936, p. 826). Special features of the Dutchess County zones that I wish to emphasize are the absence of the chlorite zone, the entry apparently simultaneously of garnet, kyanite and staurolite into pelitic rocks at the biotite isograd, and the migmatitic or syntectic nature of the sillimanite-gneiss zone. Bailey (1937) has put the orthodox British reaction to Barth's work already, and presents Harker's view that the entry of magma into the sillimanite zone is the consequence and not the cause of the sillimanite condition—admittedly, says Bailey, the concepts of Harker and of Barrow on this matter overlap considerably but, on the whole, British opinion does not favour Barrow. I have already in an earlier page briefly summarized Barrow's views on the relation between the activity of the 'old magma' and the formation of the sillimanite zone and have dealt with them more exhaustively on another occasion (Read, 1940; this volume p. 18). They are a sample of opinion on this topic; another slightly different British sample is that of Horne and Greenly (1896) who, in their pioneer account of the Sutherland migmatites, agree that granitic intrusions are closely associated with the metamorphic processes but found the schists already crystalline; 'the cause which brought about the introduction of the granite also resulted in these high and peculiar types of crystallization' (1896, p. 645).

Let us for a moment refer back again to Goldschmidt (1921) and especially to his work in the Stavanger district. He showed that there was here superposed on the regional metamorphism a

kind of thermal metamorphism around intruded granitic igneous bodies. The original rocks belonged to the regional chlorite zone and in the quartz-muscovite-chlorite-schist of this zone there were formed at a few kilometres from the contact abundant small porphyroblasts of garnet; still nearer, biotite appears by the reaction of chlorite with muscovite, then acid plagioclase porphyroblasts come in until, with the increase of the soda-lime-silica soak, augen-gneisses and granitic-looking types are produced. Here in Goldschmidt's work is a distinction between injection metamorphism and normal regional metamorphism, the former marked by transfer of material, the latter by preservation of original chemical composition. From the zoning point of view, it is to be noted that biotite is an index of a higher grade than garnet in injection metamorphism, but the garnet is a manganese variety. Injection metamorphism, Goldschmidt concludes (1921, p. 137), is very like regional metamorphism and is performed under similar physical conditions. The Barrow-Barth linking of regional metamorphism with igneous activity is becoming slightly unloosened; in later pages we examine more definite stages in this process.

This appearance of the garnet isograd before the biotite isograd seems to be no unusual event; it has been demonstrated by Anna Hietanen (Cloos and Hietanen, 1941, p. 129) in the region of the so-called Martic Overthrust in Pennsylvania and Maryland where, again, the increase in grade of metamorphism is ascribed to increase of introduced pegmatitic juices (loc. cit., pp. 17, 127, 129, 136, 153, 185, 188)—the garnet arising is an almandine. In this excellent investigation, the metamorphic rocks are grouped essentially in a number of metamorphic facies—those of muscovite-slate, chlorite-schist, biotite-muscovite-schist, almandine-schist, kyanite-schist and sillimanite-gneiss. We may now consider this question of metamorphic facies.

III. METAMORPHIC FACIES

Eskola (1922, 1925; Barth, Correns and Eskola, 1939) proposed the term *metamorphic facies* to indicate a group of rocks originating under physical conditions so similar that in materials of the same chemical composition identical mineral compositions are

produced. In other words, the mineral composition of the various members of a metamorphic facies is controlled only by their chemical compositions. Eskola (1922) held that the metamorphic facies were compatible with the Becke-Grubenmann depth-zones and studied a number of them, such as the hornfels, sanidinite, green schist, amphibolite and eclogite metamorphic facies. To illustrate Eskola's concept, we may recall his correlation with these metamorphic facies of certain igneous facies alleged to be formed under the same pressure-temperature controls and therefore equivalent. Such igneous facies equivalent to the metamorphic facies listed above are: gabbro, diabase, helsinkite, hornblende-gabbro and igneous eclogite facies. It may be pointed out that in a true metamorphic facies equilibrium should be attained—no such equilibrium can be reached in the so-called igneous facies.

Eskola (1922, p. 193) did not claim any genetic significance in the facies classification; he agreed with the authors of the C.I.P.W. system that discussions of genetic relationships are not applicable to the construction of a petrographic system. It is clear, however, that complete interpretation of all the detail seen in a facies must lead to conclusions regarding the metamorphic history of a rock—that is, to genetic conclusions—except in those rare cases where equilibrium is completely reached. Eskola (Barth, Correns and Eskola, 1939, pp. 341–3) himself realized this and decides that the study of unstable relics, posterior products and non-equilibrium associations will supply the history of the rocks. This, of course, will be of much greater geological importance than the study of true equilibrium facies.

Eskola, as we have seen, held that his facies corresponded in kind with the Becke-Grubenmann depth-zones but were more elaborate (cf. Tilley, 1924, p. 167). They were likewise identical (Barth, Correns and Eskola, 1939, p. 339) with my isophysical series of Unst (Read, 1937*b*, p. 218). A metamorphic facies includes rocks metamorphosed under identical conditions and, thus defined, they must be isophysical. Niggli (*in* Grubenmann and Niggli, 1924, p. 483) has emphasized this aspect of Eskola's proposals and has decided there is no need for a new term for what is essentially only an enlargement of the various assemblages of Grubenmann's depth-zones. On the same grounds, Niggli

(op. cit., p. 484, footnote) objects to Tilley's isograde rocks—isograde should be isophysical. The question of what isophysical implies is one of some importance; Eskola (Barth, Correns and Eskola, 1939, p. 341) has affirmed that even if the same rock results from metamorphism under P_1T_1 as under P_2T_2, then still P_1 does not necessarily equal P_2, nor T_1 equal T_2. It seems to me, however, that the isophysical condition does require P_1 to equal P_2, and T_1 to equal T_2, and, indeed, that Eskola's own picture of metamorphic facies requires this same equality.

Objection has sometimes been raised to Eskola's use of the word 'facies' itself. I see little ground for this objection, provided metamorphic is always prefixed—what might become objectionable is the use of this one word in two different senses in metamorphism. Roques (1941, pp. 488–90), for example, has proposed to employ *characteristic facies* and *ubiquitous facies* to indicate respectively parageneses that appear in one zone of the Roques-Jung system and parageneses that appear in several zones. The characters of the Roques-Jung zones of the ectinites are sufficiently marked to impress a *facies* on the majority of the crystalline schists of each zone: 'les zones se repèrent facilement sur le terrain grace à la présence de ce facies dominant' (loc. cit., p. 490). This use of facies is in the established geological tradition and must be accepted.

In what has gone before, there have been indicated various degrees of merging of the notions of metamorphic zones and facies

TABLE D

MINERAL FACIES IN SEDIMENTARY ROCKS: ESKOLA 1939

Green schist facies	Chlorite-muscovite-schist	*Chlorite zone*
	Biotite-muscovite-schist	*Biotite zone*
	Epidote-biotite-schist	
Epidote-amphibolite facies	Almandine-epidote-schist	*Almandine zone*
	Hornblende-almandine-epidote-schist	*Hornblende zone*
Amphibolite facies	Oligoclase-hornblende-almandine-schist	

and the many modifications proposed in particular sets of zones or of facies. We can without violence to either proposition decide that the two embody the same idea and are capable of correlation. Two samples of the correlations are set forth in Table D, due to Eskola (Barth, Correns and Eskola 1939), and Table E, due to Roques (1941).

TABLE E

ROQUES	*BARROW—TILLEY*	*GRUBENMANN*
Micaschistes supérieurs	Chlorite zone	Epizone
Micaschistes inférieurs	Biotite zone Garnet zone Staurolite zone	Mesozone
Gneiss supérieurs	Sillimanite zone	Katazone
Gneiss inférieurs		

I apologize for boring the specialists with these expositions, but I trust that by now even the most 'soft-rock' of geologists will be sufficiently instructed on metamorphic zones and metamorphic facies to be able to face a series of comments, criticisms and discussions upon them.

The general validity of degrees of metamorphism has been unquestioned from the earliest times of geological science. It is when these degrees become more closely defined as zones or facies and attract genetic accretions that rather deep divergences of opinion about them appear. We may now examine in detail the major diversities of opinion on these matters—they group themselves more or less clearly.

IV. DEPTH

We are first to be concerned with depth. Nowadays the use of

the term 'depth-zone' is often accompanied by a disclaimer that any special significance is to be attached to the *depth* part of it. But of course there is a significance and a legitimate one—grade of metamorphism often increases with depth. Divergence arises when it is implied that it increases because of depth, because of the increase of cover-load. In the writings of Becke, Grubenmann, Harker and other authorities there is an impression that weight of cover controls the grade, an impression particularly evident in the view that applies in some measure William Smith's First Law to the sequence of metamorphic zones. That metamorphism does not vary directly with weight of cover is indicated by the existence of thick non-metamorphic geosynclinal piles, of highly metamorphic stratigraphically younger beds overlying lowly metamorphic stratigraphically older beds, of sillimanite-zone rocks resting on lower-zone rocks, of rapid changes of metamorphic grade over a trifling vertical interval and of transgression of the stratigraphical boundaries by the isograde surfaces.

There are various kinds of depths that come into this discussion. First, there is true or *geosynclinal depth*. As we have seen, Tilley (1925, p. 109) believes it reasonable to suggest that the metamorphism of the Scottish Dalradian rocks was essentially acquired at a stage in their history when 'the disposition of the zones was in approximate accord with the depth of burial of the sediments', and he and Elles (1930) argue from this that inversions of the metamorphic zones can be correlated with stratigraphical inversions. According to these workers, discordances between stratigraphical and metamorphic horizons are the result of irregularities due to the configuration of the geosynclinal floor. Staub (1920) has likewise related metamorphic variation in the Alps with original geosynclinal variation.

Considerations already presented indicate that this simple relationship, either in general or in detail, does not hold. Termier (1912) long ago admitted that though regional metamorphism was concerned with depth and its most important developments were associated with geosynclines, still something more was needed—in his opinion, the arrival of juvenile vapours from the depths. Again, Goldschmidt (1912*b*), while emphasizing the role of cover pressure in the South Norwegian Caledonian geosyncline, brought in hot plutonic bodies to assist in the zonal metamorphism

of the contents of the trough. Eskola (1915, p. 116) held that depth is no factor, the rate of increase in temperature downwards being modified by the presence of 'magma masses which play such an important part in rock-metamorphism'. Turner (1933*a*, p. 252) points out that if metamorphism is a purely dynamo-thermal process dependent on folding and burial, there should be parallelism between isograds and regional strikes—that this is not so may be due to the influence of subjacent magmas. Lastly Cloos and Hietanen (1941, pp. 136–7) record transgression of all boundaries by isograds and metamorphic facies and again call in the help of intrusions. These are samples of a world-wide opinion.

There may thus be another kind of depth to be considered which we may call the *magmatic-migmatic* depth, that determined by the height reached by igneous intrusions or by the migmatite front. By this ascent, heat and fluids are brought into higher levels of the crust to become the most potent agents of regional metamorphism. As we have already noted, French geologists working in the Massif Central consider that there is a close relation between the rise of magma (e.g. Demay, 1942) or the rise of the migmatite front (Jung and Roques, 1938; Roques, 1941) and the production or modification of the metamorphic style. There may be ectinite zones distinct in place and time from migmatite zones. This complicated matter we attempt to unravel later.

There is yet another style of depth: namely, the *tectonic* depth. The aspect of this concerning us has to do with the effects of increasing the cover-load not by burial in a geosyncline but by piling higher tectonic elements on lower to produce a blanket. This is a depth whose effects should be best displayed in the Alps but, so far as my study goes, its demonstration is not a simple and straightforward matter. We may contrast in general the cataclastic and mylonitic character of the higher structural elements such as the East Alpine nappes with the recrystallized character of lower elements such as the Pennids, as has been done by Niggli (*in* Grubenmann and Niggli, 1924, p. 232), Staub (1920), Reinhard (1935), Cornelius (1921) and others, but how far this contrast is to be interpreted as due to plain depth is not clear. Cornelius (1921) has compared the 'wax' or 'rubber' tectonics of the Pennids with the *Gleitbretter* tectonics of the higher East Alpine nappes: he shows how the metamorphism increases from high to lower

structural elements, and how the same change takes place where pitch leads to depression, as displayed by the high-grade gneisses in the trough between the Gotthard and Tessin massifs. Cornelius concludes that regional metamorphism varies with the load or, better, with the increase of temperature because of the increase of load due to the piling up of the nappes. He realizes that there are certain difficulties in this view, especially those raised by the high-grade metamorphism of roots.

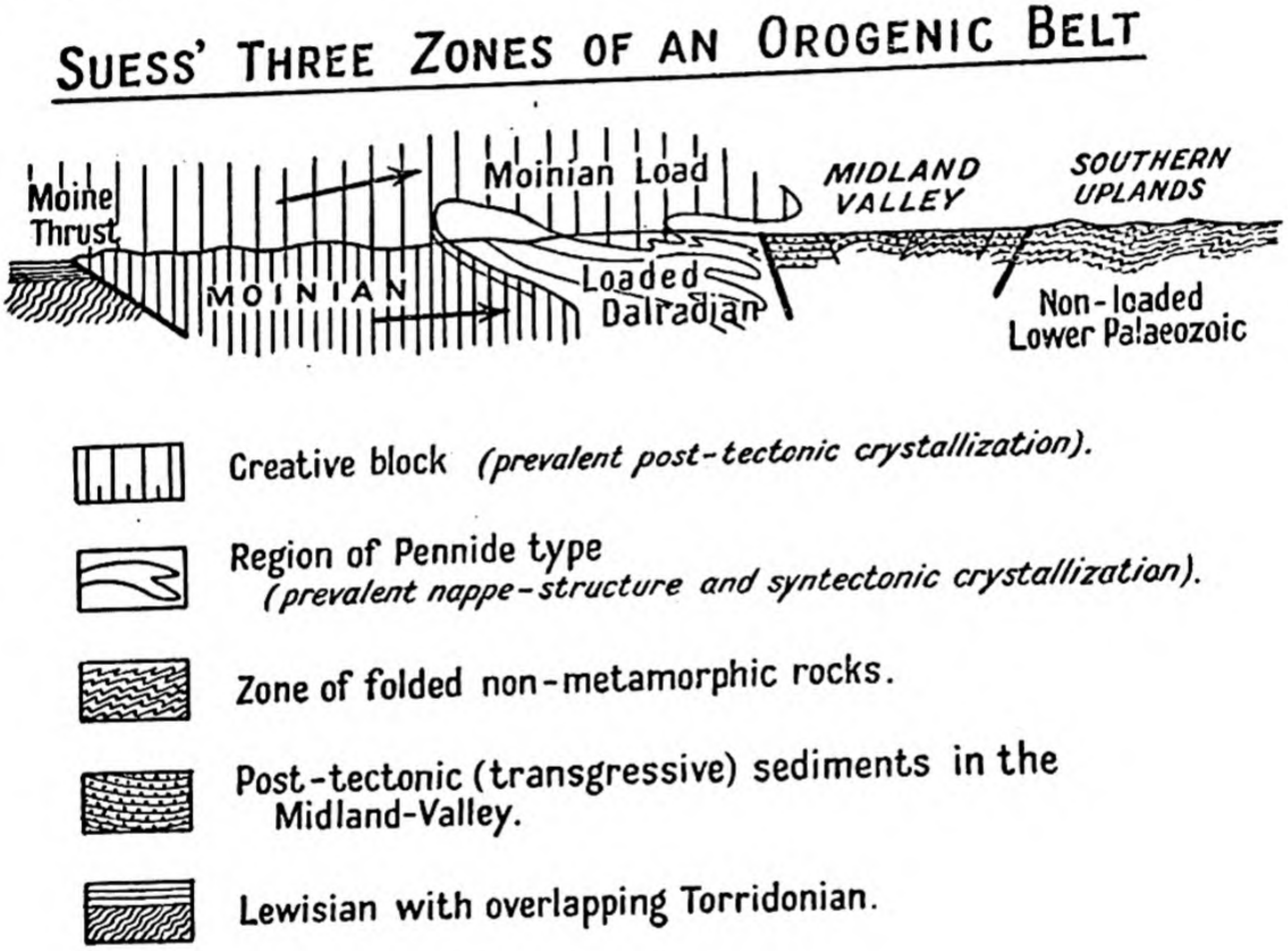


FIG. 1.

This blanketing effect recalls the interpretation of the metamorphic and tectonic relationships in the Hercynian belt of Central Europe advanced by F. E. Suess (1926, 1934, 1936, 1938*b*; and Read 1940, pp. 241–2; this volume pp. 28–30). Suess sees three great zones in an orogenic belt—(*a*) the *non-loaded*, an outer non-metamorphic zone with fold and nappe tectonics, (*b*) the *loaded*, a central zone of metamorphic rocks with a similar tectonic style, and (*c*) the *load*, the zone of intrusion tectonics. In Fig. 1 is given Suess's application of his views to the Scottish Caledonids.

The load, as the Moines, has a katazonal metamorphism not related to depth but to igneous intrusion, the loaded Dalradian has a patchy meso-epizonal metamorphism produced by deformation during the passage over it of the Moine creative block which, by thickening the cover, dams back the heat supply. I must baldly state that, for my part and so far as the Highlands are concerned, I do not believe a word of it.

Suess considered that the Grubenmann zones, viewed as a regular series of depth-zones arranged one above the other, could no longer be contemplated; they are separable only on grounds of tectonics and not of depth. He held that mica-schists were not intermediate between gneiss and phyllite but were dominantly polymetamorphic and associated with deep-seated thrust-zones. But whilst he dispensed with depth-zones and geosynclinal depth, it seems to me that he did not dispense with all depth—in the loaded zone at least the thickness of the cover controlled the metamorphic style.

V. PRESSURE AND DEPTH

We may now look into certain aspects of the variation of pressures of different kinds with depth. First there is the Volume Law to be considered. I have already hinted at its difficulties. If any strict arithmetical application of the law were true, there should be no felspars in the metamorphic rocks. In this difficulty, Becke found comfort in the contemplation that albite, which disobeyed the law the least, was the commonest felspathic constituent of the metamorphic rocks. This has always seemed very cold comfort to me, in that albite is the felspar of the shallowest zone and that the felspars themselves are of very different compositions. Calculations that ignore the crystal structure of the silicates are meaningless.

Sederholm (1926, pp. 119–30) would have little to do with the Volume Law and scorned the definition of a metamorphic rock as one that 'came under the influence of the Volume Law'. According to him, the importance of pressure in metamorphism has been overrated—its chief function is not to create new minerals and rocks but to break old ones down. Garnet is not to be employed

as a geological barometer, as Fermor and Eskola suppose, since Sederholm finds garnet to arise chiefly in rocks rich in solvents or those influenced by granitic ichors.

The Volume Law would be valid for increase of pressure at constant temperature in a closed system—but such conditions can be but rarely realized. The movement of material and the change of composition during metamorphism make its general application impossible. We might, however, expect that some relationship should exist between the operative pressure and the closeness of ionic packing of the minerals formed and that the reticular density of minerals should play a role in the shapes assumed. This latter point will be dealt with later, and here we may recall the investigations of Fairbairn (1943) on the *packing index* of minerals, so far as metamorphism is concerned. The packing index is the ratio multiplied by 10 of the volume of ions to the volume of the unit cell. Fairbairn admits that at present his values can be semi-quantitative only but, on the other hand, holds that the study of packing index is worth making now in spite of lack of precision in certain of the determinations. The packing indexes of common metamorphic rocks, as calculated by Fairbairn, are listed in Table F.

TABLE F

PACKING INDEX OF METAMORPHIC ROCKS
(Fairbairn, 1943)

Sillimanite-gneiss Albite-schist	5·1	Kyanite-quartzite Andalusite-gneiss Chloritoid-schist	5·4
Anthophyllite-gneiss Amphibolite Scapolite-gneiss	5·2	Chlorite-schist Garnet-gneiss Staurolite-gneiss	5·5
Green schist Mica-schist	5·3	Glaucophane-schist Eclogite	5·9 6·2

Comparisons without consideration of mineral and chemical compositions are not warranted, but it seems to me that, except in the case of eclogite, the effects of hydrostatic pressure are not

clearly shown. I do not propose to enter into the eclogite problem, and have only to record that this rock is distinguished from all others by its high packing index and by the high indexes of its constituent minerals, e.g. pyroxene 6, garnet 6.5, and kyanite 7; Fairbairn concludes that 'circumstantial evidence points to high pressure as a controlling factor in the formation of eclogite, as it would indeed be a coincidence if development of this unique mineral association were controlled by thermal or other factors alone' (loc. cit., p. 1364)—this conclusion being almost the only geological result that he considers clearly indicated from his study.

It would be unfair not to display Fairbairn's computations for an isochemical series. Table G gives his results for the derivatives of diabase and may indicate to the hopeful some measure of agreement between packing index and grade.

TABLE G

PACKING INDEX OF DERIVATIVES OF DIABASE
(Fairbairn, 1943)

Norite-granulite	5·3
Green schist	5·3
DIABASE	5·4
Essexite-hornfels	5·4
Epidote-amphibolite	5·5
Amphibolite	5·5
Glaucophane-schist	5·9
Eclogite	6·2

VI. STRESS AND ANTISTRESS

I propose now to examine another aspect of pressure and depth which is presented by the division of metamorphic minerals into stress and antistress groups. It will be convenient if I preface this discussion by yet another table based upon Fairbairn's calculations. Table H lists some of the stress and antistress minerals according to Harker, with Fairbairn's packing index attached to each.

Fairbairn (loc. cit., p. 1366) has grouped the metamorphic minerals in a different way into those formed at low or intermediate temperatures and those formed at high, and concludes

that the stress minerals are low-temperature forms with a high packing index and the antistress minerals are high-temperature forms with a low packing index. Neither part of these conclusions appears valid to me from any evidence derivable from Fairbairn's figures and we may now proceed to discuss the whole question of stress and antistress groups. Place is concerned here, as the upper depth-zones are held to be the seats of strong stress, the lower of weaker stress.

TABLE H

PACKING INDEX OF STRESS AND ANTISTRESS MINERALS
(Harker, Fairbairn)

Stress Minerals		*Antistress Minerals*	
Kyanite	7·0	Andalusite	6·0
Sillimanite	6·2		
Albite	4·9	Anorthite	5·0
Sericite	5·5	Orthoclase	5·0
Amphibole	5·7	Pyroxene	5·9
Epidote	6·1	Wollastonite	5·2
		Scapolite	5·2
Chlorite	5·8		
Staurolite	6·7	Cordierite	4·7
Chloritoid	6·2		
Talc	5·6		
Serpentine	5·4	Olivine	5·9
Glaucophane	5·7		

The division of metamorphic minerals into stress and antistress groups is due especially to Harker (1919*b*, 1932). In his Presidential Address of 1918, he held that certain minerals, such as kyanite anthophyllite and antigorite, 'were so exclusively associated with crystalline schists that we may reasonably infer shearing stress to be necessary for their production'. This inference, of course, depends fundamentally on the assumption that shearing stress is necessary for the production of the crystalline schists. This assumption could be shown to be true, Harker proposed, on geological evidence; the crystalline schists possessed a texture obviously controlled by stress and, therefore, the components delineating the texture were similarly controlled—they were stress minerals. He contrasted the stress minerals of the crystalline

schists with the antistress minerals of the thermal aureoles and emphasized the contrast by recalling that the latter group contained those minerals that had been artificially made in the laboratory.

The stress and antistress groups have been extensively applied as indices of the nature of physical controls during metamorphism and thus of the metamorphic history. The silicates of aluminium are especially employed and in many terrains elaborate histories have been worked out from them (see, for example, Sugi, 1931; Suzuki, 1930; von Eckermann, 1936, Billings, 1941; Cloos and Hietanen, 1941). I recall, however, that in the andalusite-schists of Banffshire, andalusite, staurolite, sillimanite and cordierite appear to be comfortably contemporaneous (Read, 1923, p. 59) and that, in Finland, Eskola (1927, p. 70) has recorded the intercalation of andalusite-schists with staurolite-schists but has never observed andalusite and staurolite in the same rock.

It becomes of interest to inquire, therefore, what validity there is in the stress-antistress grouping. There is a vast array of evidence to show that all the celebrated stress minerals can be formed under conditions of no stress. The inability of man to make these minerals indicates little in this matter. Let us sample the evidence. Du Reitz (1938) has shown that sillimanite and kyanite are due to injection processes and that kyanite occurs where the injection is strongest. Suess (1937, p. 46) has pointed out that many stress minerals are found in the dead spaces of folds. Erdmannsdörffer (1928) has described kyanite, associated with andalusite, as joint fillings. Barth (Balk and Barth, 1936, p. 789) has recorded tiny kyanite crystals lining shear-planes and concludes that they have crystallized directly from solution. I have described quartz-kyanite veins and quartz-chlorite veins from Unst (Read, 1933, 1934*b*), whilst staurolite has been recorded from similar environments. Stress anthophyllite is often associated with antistress cordierite, whilst chloritoid, staurolite, glaucophane and many other stress minerals are found in the antistress environment of thermal aureoles. Davidson (1943) has described gneisses with cordierite and kyanite from Rodil, Harris. A number of stress minerals are constituents of pegmatites; Karpoff (1946) concludes, 'le disthène ne peut être rangé d'une façon trop catégorique dans la grande classe des "stress minerals" '—an expression

indicating, I feel, that the English have no monopoly of the gift of understatement. Likewise, sillimanite is known as a constituent of pegmatites and the conditions of its formation, as Williams (1934) has shown, are pneumatolytic and not stress. Lastly, my fellow-student of the Sutherland migmatites Janet Watson (1948) has demonstrated that sillimanite can occur as a metasomatic product of granitization and concludes that sillimanite is not formed 'only as a result of changes in the physical conditions of metamorphism'.

On many counts, therefore, it seems to me to be unwise to consider any given mineral as indicative of stress or non-stress. It is particularly unwise to use, as is often done, certain minerals as touchstones in deciphering metamorphic history—this history is not to be read in this manner.

VII. PLACE ON THE MAP

The metamorphic geologist, like any other geologist, is in the ultimate concerned with a geological map and we have to inquire how zones and facies as concepts of place can be dealt with by the field-worker. What the metamorphic geologist should map is a problem for profound debate. He has often, like an expert juggler, to keep three or four balls in the air at one time: he has to map not only what he takes to be the stratigraphy, but maybe three or four lithologies—the original pre-metamorphic lithology and later ones recording the diverse episodes of the metamorphic history. It is the pre-metamorphic lithology that is almost never mapped, because of notions, mostly erroneous, concerning the time-relations of tectonics and metamorphism. I should rejoice to see, for example, a map of the Dalradian rocks in their original non-metamorphic sedimentary and igneous condition, as I am certain it would help in the attack on this tough problem.

It is undoubtedly difficult—as I know from my experiences in the polymetamorphic rocks of Unst in the Shetlands—to place the boundaries of metamorphic zones and metamorphic facies on the map. I have already noted this difficulty with reference to Jung and Roques's query of what to do with epizonal serpentine in mesozonal gneiss. The answer to this one is easy: the serpentine is

mapped as serpentine and the gneiss as gneiss and then the question of both place and time considered. It is the time dimension in metamorphism that makes the mapping of metamorphic rocks so difficult and so fascinating—we need a number of maps to be tied together almost as a film. Metamorphic rocks can hardly be mapped as inanimate objects. Let us consider an example.

In the Dalradian there is often difficulty, as Tilley (1925, p. 103) has noted, in mapping the biotite zone because of the development of retrogressive chlorite. In the Ardlui district of the Dalradian, J. de Lapparent (1935) has gone so far as to suggest that the Dalradian is polymetamorphic and that a chlorite metamorphism has been superposed on a garnet metamorphism. It becomes debatable, therefore, as to what is being mapped in either the garnet or the biotite or the chlorite zones of Tilley. We may here be dealing with a diaphthoretic modification of the most profound time significance—a significance as great as that of the phyllonites of the Alps and elsewhere. As I have noted in another place, the chlorite zone is often difficult to accept as the first step in a series of progressive metamorphic changes. I may recall that Turner (1933*a*, p. 249) has described a diaphthoresis giving retrogressive chlorite-grade rocks in New Zealand, and it is possible that a time-study of the chlorite zone generally would give results of great interest. Time is just as important in metamorphism as place.

So much for metamorphic zones—now let us look at metamorphic facies.

VIII. THE ORIGINAL COMPOSITION EFFECT

If, for example, metamorphic facies are primarily useful, as Eskola proposes, for the construction of a petrographic system without genetic significance, then they should be capable, as they often are, of expression on a geological map. But, on occasion, such mapping has led to the questioning of the whole validity of zones and facies. Thus Suzuki (1930), as a result of his detailed petrographical and chemical study of the crystalline schists of Shikoku in Japan, came to the conclusion that it is what I may term the *original composition effect* that controls metamorphism.

Suzuki describes a 'green schist' assemblage in the Besshi Series that includes such diverse types as chlorite-schist, amphibolite, zoisite-amphibolite, garnet-amphibolite, kyanite-amphibolite, eclogite, glaucophane-schist, actinolite-schist and serpentine. Here is a congeries of facies. By detailed chemical study of the minerals and rocks, Suzuki demonstrated that each type had its own special composition. There were no palimpsest or relic textures, no transitions from one mineral to another and, since there were no differences in physical controls, chemical composition determined the assemblage of facies. I quote from Suzuki (loc. cit., p. 103): 'the re-establishment of equilibrium is to be regarded as complete, because, on the one hand, the rocks in question are made up entirely of recrystallized minerals and on the other the chemical composition of each rock is characteristic for its mineral assemblage. There is no case in which the classification based on chemical composition stands in contradiction to that based on mineral assemblage.' The rocks of the series are isophysical and the original composition effect seems to be entirely responsible for the profound facies-differences exhibited by the various rock types. If we accept Suzuki's careful work at its face value, we have to make the metamorphic correlation of chlorite-schist with amphibolite and eclogite, for example, and the whole concept of zones and facies becomes imperilled. I know of no discussion of Suzuki's results; criticisms, some of them maybe discourteous, could be easily made, but they would be uninformed.

In support of Suzuki's position, it must be noted that K. Sugi (1931) has shown that, in the Misaka Series of Japan, different mineral assemblages can arise directly from the initial igneous rock. Further, Turner (1933*b*) has displayed how the assemblages in green schists are influenced by such factors as metamorphic grade, initial composition and changes in composition: 'in the green schists, variation in chemical composition is partially responsible for variation in the course of metamorphism' (loc. cit., p. 535).

Water and carbon-dioxide, especially at low grades, are important factors in the facies concept in that they affect the operation of metamorphic diffusion. A general discussion has been given by Turner (1935*a*, 1938*a*), who shows that there are several distinct facies of green schists—all of the chlorite zone—whose

production depends upon the amount of water and carbon-dioxide present; the controls are indicated in Table J (Turner, 1935*a*, pp. 420–1; see also Hutton, 1940).

TABLE J

CONTROLS OF THE GREEN SCHIST FACIES
(Turner, 1935*a*)

Facies	*Control*
Actinolite-albite-epidote-chlorite	CO_2 absent, H_2O insufficient to allow chloritization of actinolite.
Chlorite-albite-epidote-calcite	CO_2 present, H_2O sufficient.
Chlorite-albite-epidote	CO_2 absent, H_2O sufficient.
Chlorite-albite	CO_2 absent, H_2O abundant, removal of Al and Ca.

As a result of his intensive studies, Turner (1935*a*, pp. 415–7) has advanced a modified conception of the green schist facies, illustrated by reference to the derivatives of basic igneous rocks. It becomes clear that the change gabbro into green schist can take place under a variety of conditions—under falling temperature in the presence of abundant water (Turner), under high temperature (Vogt, 1927, pp. 407–10, 515, 516), by hydrothermal action without stress (Hess, 1933) and, finally, under stress conditions (Eskola, 1925; Sugi, 1931, pp. 89–142). It seems, therefore, that however desirable metamorphic facies may be as a classification principle, it is not necessarily of any genetic significance. The rocks remain very dead.

Even with the simplifying assumption that in metamorphism no transfer of material takes place, the application of zoning or facies becomes difficult to the field worker on account of the very different reactions by rocks of different initial compositions. For a simple case, we can again refer to Turner's work on the chlorite zone of greywackes, his correlation being presented in Table K.

If the work of Suzuki, already referred to, is valid we have another excellent example of the initial composition effect. The observations of Corin (1930, 1932) in the Ardennes indicate that the development of metamorphic zones is there very irregular, depending fundamentally on the sedimentary facies of the rocks, so that the normal zonal index-minerals appear in variable order.

TABLE K

ORIGINAL COMPOSITION EFFECT

New Zealand greywacke zones	*Highland pelite zones*	*Grubenmann zones*
Chlorite	Chlorite and outer biotite	Epizone
Biotite	Inner biotite and outer almandine	Mesozone
Oligoclase	Middle and inner almandine and staurolite-kyanite	Mesozone

The original composition effect can be illustrated also by the reluctance to reconstitution of rocks containing finely divided carbon. Bailey (e.g. in Tilley, 1925, discussion) has long maintained that the absence of garnet in the Ballachulish Slates was due to their carbon content. In his Sulitelma studies, Vogt (1927, pp. 483–4) has indicated that finely divided carbonaceous matter in the black slates of the chlorite zone has impeded the reactions that would result in new mineral formation; even in higher stages of metamorphism rocks are conspicuously fine-grained when they contain much carbonaceous matter. Eskola (1932*a*, p. 27) has recorded a similar impedance.

The difficulties arising from the variations in original composition were of course realized by Becke (1921) in his first discussion of the facies principle. He pointed out that whilst the amphibolites of the Waldviertel of Austria had the paragenesis required to classify them with the amphibolite facies as defined by Eskola from the amphibolites of the Orijärvi district of Finland, the associated lithological types had different assemblages in the two regions. The Austrian limestones had no wollastonite but plenty of tremolite and sahlite, and the pelites had no cordierite—viewed by themselves these rock types would not be in the amphibolite facies at all.

These, and many similar examples, indicate in my opinion that zone and facies concepts must be used in very general and wide terms and for somewhat limited areas. A regional metamorphic study that deals with zones or facies must include a presentation

of what I have called *metamorphic correlation* (Read 1937*b*)—the correlation of assemblages of varied chemical composition. To illustrate the importance of metamorphic correlation, I cite certain results from Unst where pelitic epischists are characterized by retrogressive chlorite whilst in interbanded siliceous rocks biotite, though mauled, still persists. The proposals of zones and facies are vastly more difficult of application in the field than their academic presentation would lead one to expect.

IX. EQUILIBRIUM

The prime requisite for a metamorphic facies is the attainment of equilibrium. Rarely, fortunately, can a rock have achieved this over any large space—fortunately, as I have said, because non-equilibrium assemblages, relic minerals and textures, transitions and other evidences of a time-sequence will indicate that the majority of our rocks are very much alive; they have that dimension of time that the *pétrographie de tiroirs* can neglect.

Let us grant, for the moment, that equilibrium can be reached in rocks undergoing metamorphism without change of chemical composition and that the facies concept can be applied to them. We have to inquire whether equilibrium can be attained in systems changing in composition and whether such systems occur in metamorphism. Eskola (1939, p. 344) himself has admitted that in open systems metamorphic facies will be difficult of application. Movement of material by metasomatic activity, by metamorphic diffusion and differentiation may markedly change the conditions of transformation.

It is certain that in metamorphism changes of composition occur, though it is the custom to ignore them. I refer back to my remarks on Turner's consideration of the green schist facies and its dependence on water and carbon dioxide. It is usually said that the green schists are rocks of dolerite composition—but this they cannot be; they must change their original composition and acquire water and carbon dioxide to make the very minerals—chlorite, epidote and calcite—characteristic of their facies. Becke (1921, pp. 227–8) holds that even under such conditions, the principle of equilibrium can be applied. In the green schists,

for example, equilibrium is reached when all the Mg-, Fe- and Al-bearing silicates corresponding to the governing temperature, pressure and water-vapour pressure, are altered into chlorite; in the case of biotitic green schists, equilibrium is reached when in conformity with concentration of incoming K-ions and with the circumstances of temperature, pressure and vapour pressure of carbon dioxide and water, the same silicates are converted into biotite.

This concept seems to me to break down the ideal scheme of Eskola and to necessitate the establishment of equilibrium in steps, as it were, on a gradient of many qualities—temperature, pressure and concentration. The original composition, at the receiving end of the operation of transfer, becomes once more of fundamental importance. We have, therefore, to inquire whether metamorphic systems are closed or open, whether influx and efflux of material takes place in metamorphism.

X. CLOSED OR OPEN SYSTEMS

The old idea that movement of material was possible and indeed necessary in metamorphism gave way before Rosenbusch's results on the Barr-Andlau granite-contact. The standard opinion became that metamorphism left rocks with their original chemical composition unchanged. The whole question of retention of original composition is really one of degree. No one nowadays denies some transfer of material—the difficulties arise with the consideration of how much, how far and how. To begin this discussion, we look into a few authoritative statements.

Grubenmann (1910, p. 45) considered that metamorphism in the stricter sense requires that the chemical composition of the original rock is essentially unchanged. He admitted, however, that the preservation is not absolute and suggested that the degree of change varies with the position of metamorphism—in the upper zones, the presence of abundant channels permits considerable transport of material over great distances, whereas in the deeper zones, with less pore space, flow is less and what transfer goes on does so by diffusion. His general conclusion is that the greater the earth-depth, the more perfect will be the preservation of

original composition—this is indicated by the occurrence of metamorphic rocks with the compositions of the variety of igneous rocks in the deeper zones. But Grubenmann (1910, p. 46) himself goes on to cite the case of the Tremola Series in the St. Gotthard region where the chemical composition of the rocks undergoing metamorphism has been completely changed by the addition of soda.

Again, Harker (1932, p. 2) commences his great study of metamorphism by feeling 'able to assume without sensible error that the rock, and even any small part of the rock, suffers no change in total composition during the process'. To him, metasomatic processes, or change of substance, are of little importance compared with metamorphism or change of form.

That change of composition does occur in plutonic operations has now been generally realized, but it has been utilized either to break up the plutonic series or to subdivide the metamorphic rocks themselves. Balk and Barth (1936, pp. 834–5) have concluded from their study of the rocks of Dutchess County that 'During a period of orogenesis, the down-folded Palaeozoic sediments . . . were heated and stewed in liquids of magmatic and anatectic origin, thus forming the rock series, (sediment) ⟶ slate ⟶ schist ⟶ gneiss ⟶ augen gneiss ⟶ (intrusive granite). In this series, the chemical composition changed gradually from that of an argillaceous slate, through schist and gneisses, to granite. There is no difficulty in describing both the slate and the schist phases as metamorphic sediments. But what should one call the gneisses?' The gneisses are interpreted as of mixed sedimentary and igneous material and Balk and Barth prefer to call such rocks of mixed origin *syntectic* and 'reserve the term "metamorphic" for recrystallized rocks derived from pre-existing sedimentary or igneous material, without essential anatectic or metasomatic alteration' (loc. cit., p. 838). They state that these two rock-groups 'of essentially different mode of origin have been classed together' and that, as a consequence, 'the study of both metamorphic rocks and syntectic rocks has been hampered in the past'. They feel that 'a boundary between such processes and metamorphism proper must be drawn somewhere' (loc. cit., p. 838). For my part, I do not experience any such feelings. On their own showing, Balk and Barth are dealing with a continuous series of rocks and

it seems preferable to me to emphasize the genetic unity of this series rather than to break it up into alleged different parts. The principle underlying the separation of metamorphic and syntectic rocks is that, in the former, any liquid present acts only as a catalyst whilst, in the latter, it acts metasomatically; it is metabolism contrasted with metasomatism. For reasons that we discuss immediately, I hold that some movement of material is a part of all metamorphic processes, whether the products be low or high in grade.

We can compare this proposed distinction between metamorphic and syntectic rocks with that between metamorphism and injection metamorphism advanced by Goldschmidt (1921) for the Stavanger rocks, to which I have already referred. In injection metamorphism, there is a clear transfer of material; in plain metamorphism, the original chemical composition is preserved. In Stavanger, the injection metamorphism was superposed on the regional metamorphism but, according to Goldschmidt, the two operations were performed under very similar physical conditions. But still the two were distinct events—a circumstance recalling the views already noted of Jung and Roques on ectinites and migmatites as constituting two separate groups of the metamorphic rocks, the first showing no visible accession of felspathic material, the second often exhibiting an intense influx of such material. Metamorphic to Jung and Roques thus includes the metamorphic and syntectic of Balk and Barth. I hasten to recall that the French workers propose a separation in place and in time of their two groups of metamorphic rocks—a separation we examine later.

It is evident that in this aspect of plutonism, two questions have to be considered. The first is concerned with the constancy or otherwise in chemical composition of rocks during metamorphism, the second with the more geological problem of place and time in the metamorphic and syntectic rocks, the metamorphic and injection-metamorphic rocks, or the ectinites and migmatites.

XI. COMPOSITION SERIES

In innumerable studies of series of metamorphic rocks it is stated on the basis of somewhat random analyses that there is no

serial change of bulk chemical composition. This is a very difficult matter to establish except by very careful work on a single bed or by the consideration of a large number of analyses of metamorphic types. Work along these lines is just beginning and will be greatly facilitated when a collection of analyses of metamorphic rocks is

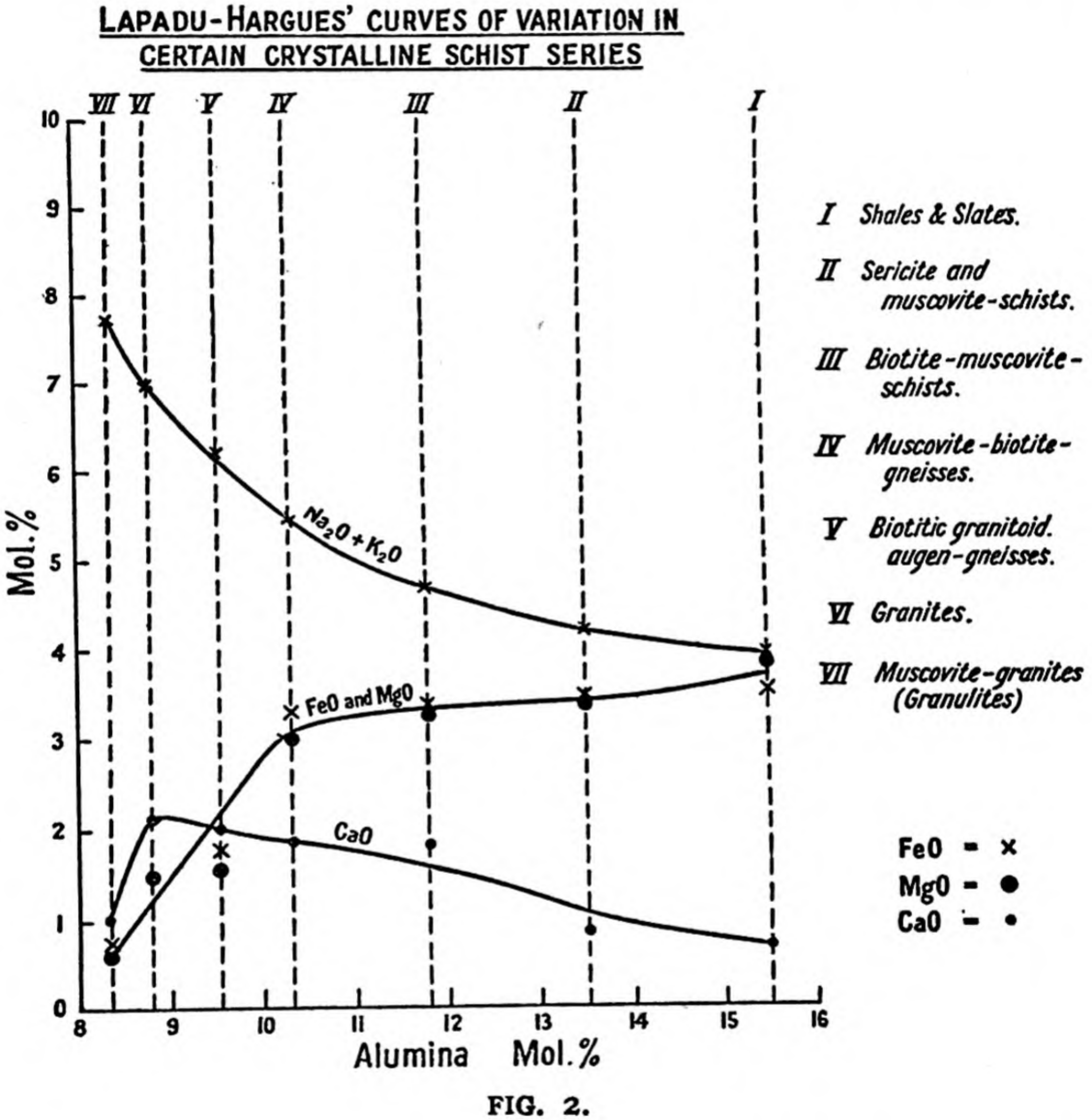


FIG. 2.

available similar to Washington's *Tables* for the igneous rocks. Brammall (1933; see also MacGregor and Wilson, 1939, Fig. 1) has made a start and in his normative variation diagrams has given an indication that in the reconstitution process of shale to phyllite to mica-schist there is an increase of soda content relative to potash: the metamorphic rocks lie nearer the albite corner of his

triangle than do the sedimentary rocks. This is in accord with a great many studies, of which I need only specify Barth's (1938) account of the progressive metamorphism of the sparagmites of southern Norway; here a soda-metasomatism is indicated—'a process very common to regional-metamorphic rocks', as Barth says (loc. cit., p. 54).

In this matter, and in what follows, it is important to consider

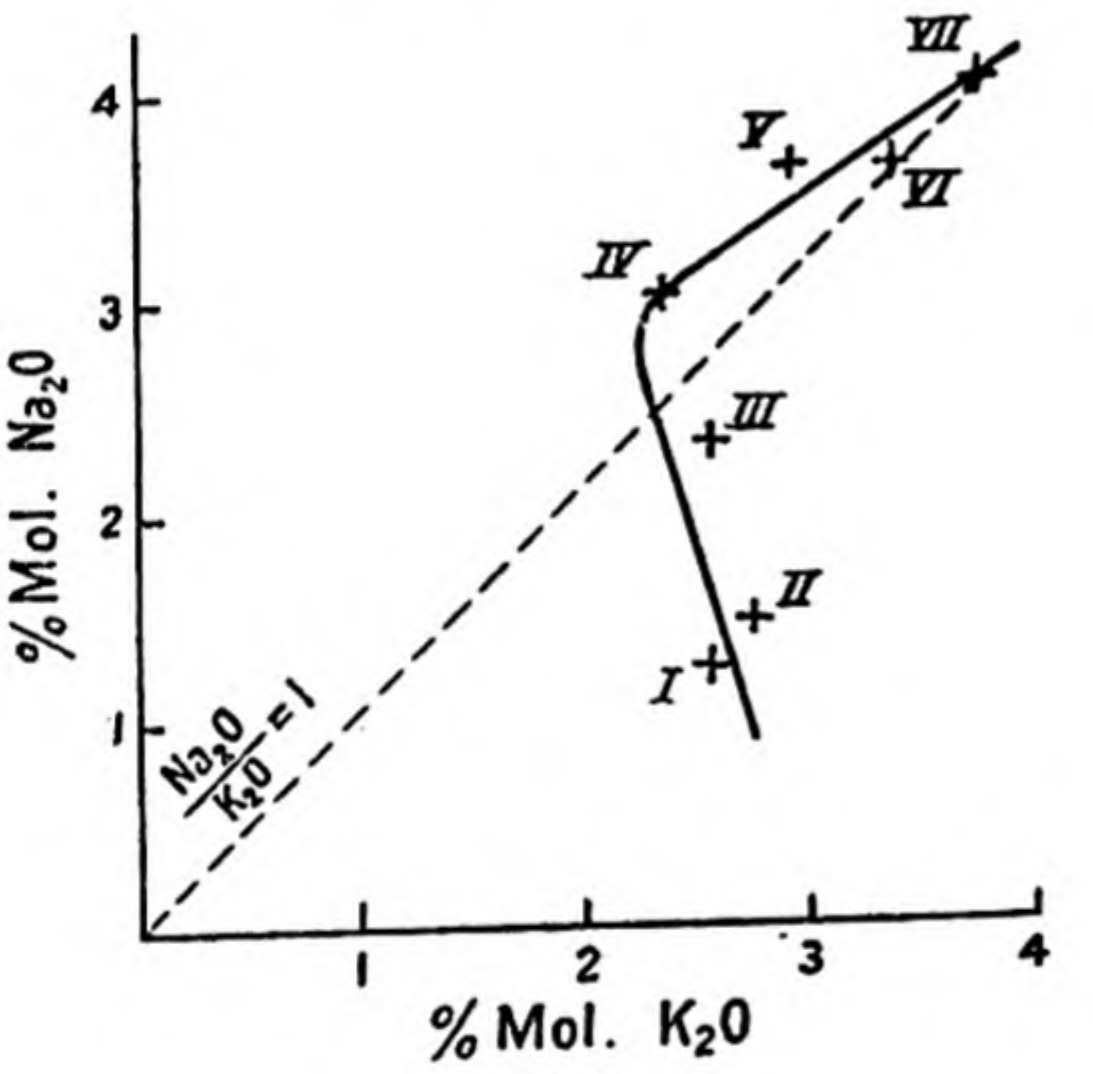


FIG. 3.

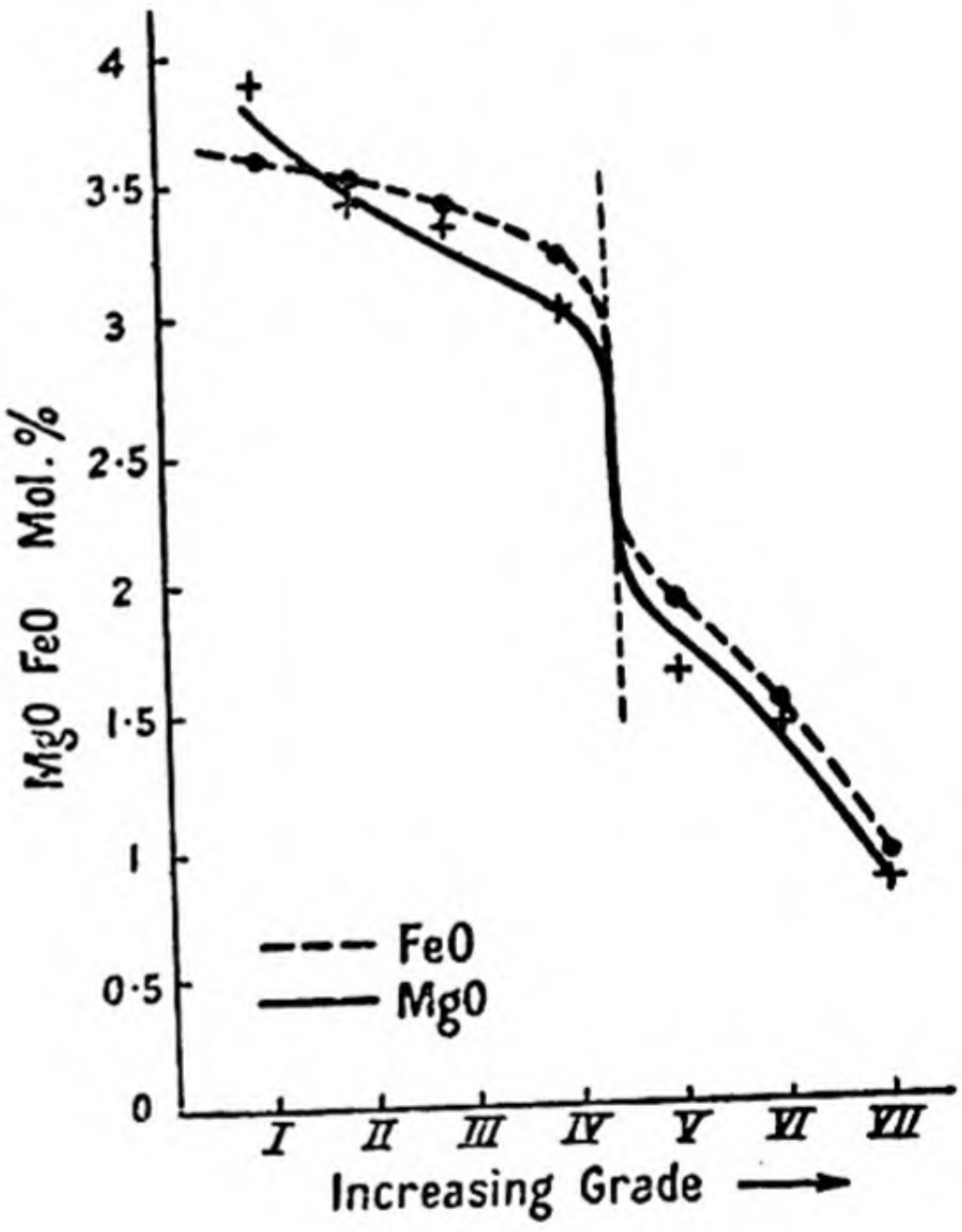


FIG. 4.

the recent essay of P. Lapadu-Hargues (1945)—an essay that may play a fundamental part in the progress of plutonism. Lapadu-Hargues deals with the variation of the molecular percentages of the chief oxides in groups of analyses of seven classes of rocks—slates, sericite- and muscovite-schists, biotite-muscovite-schists, muscovite-biotite-gneisses, biotitic granitoid augen-gneisses, granites, muscovite-granites (granulites of the French). His chief results, indicated in Figs. 2, 3, and 4, can be summarized as follows:

1. *Alkalis: alumina.* With increase in grade the ratio of alkalis to

alumina approaches unity, which is, of course, the felspathic ratio (Fig. 2). The conclusion is that there has been a progressive influx of alkalis coming from the deeper zones.

2. *Soda: potash.* There is a soda accession in the low grades, and a dominantly potash accession in the higher (Fig. 3). Both come from a deep source and therefore the soda migrates farther than the potash which remains localized in the deep zones, its upper limit being the transition between the gneisses and the schists—'c'est le point de transformation relatif à l'apport differentiel des métaux alcalins dans le phénomène du métamorphism' (loc. cit., p. 290).

3. *Magnesia: iron-oxides.* The variations of magnesia and iron-oxides go together—iron and magnesium are 'métaux frères'. There is a general decrease with grade and therefore none is imported; a break in the curve at the division between the gneisses and the augen-gneisses indicates that the oxides have been concentrated in the upper zones and become rarefied in the lower—this is thought to be an operation of the basic front about which I have something to say later (Figs. 2 and 4).

4. *Lime.* There appears a progressive increase of lime up to the granite (VI) stage, comparable with the behaviour of soda (Fig. 2).

There is proposed from these results a calc-alkali influx, constant and progressive, tending to produce a common product as the change proceeds, together with an iron-magnesia migration from the interior of the series. The smallest ions migrate the farthest; the potassium ion, for example, being large is confined to the internal zones. Lapadu-Hargues's final results can be represented in Fig. 5. The dependence of migration on atomic radii was earlier invoked by Backlund (1936, p. 340) and has become of importance in the discussion of granitization and allied processes.

The work of Lapadu-Hargues is but a beginning. His curves are based upon analyses that appear to me to be insufficient in number and haphazard in choice. This study is one requiring multitudes of analyses statistically considered. Though I am entirely in sympathy with his findings, I feel compelled to make these reservations. The future development of this kind of investigation may perhaps best depend on the calculation of distribution and change of atoms rather than of oxides. Lapadu-Hargues himself supposes that the mobile metallic ions move through a passive framework

of SiO_4: AlO_4 which remains relatively inert. I have recently suggested that the large oxygen atoms of the silicate lattice remain more or less in position whilst the small migrating atoms drift through. It would be of surpassing interest to study the variation in the plutonic rocks keeping the oxygen-content stable; ion and not oxide variation is required.

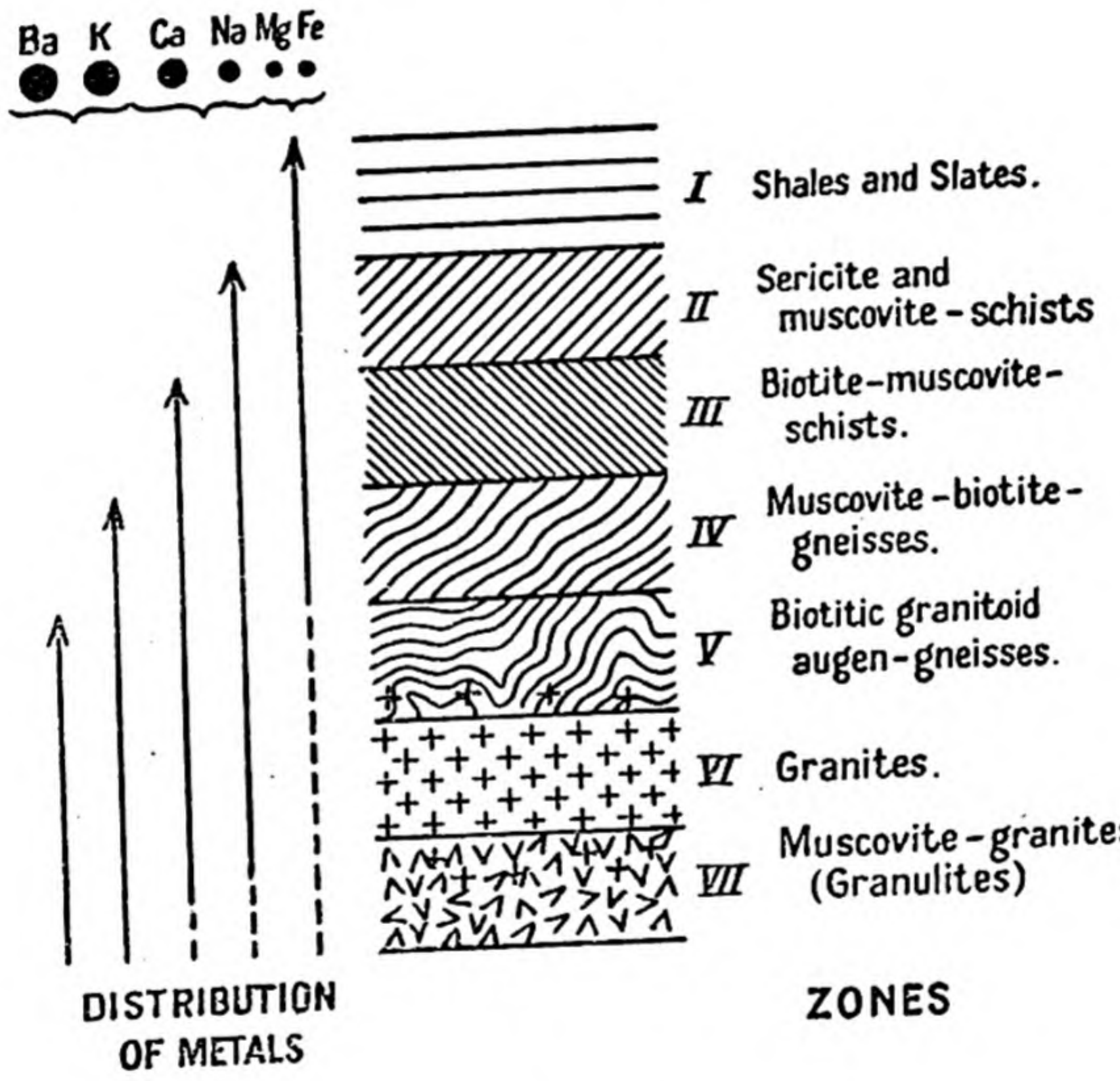


FIG. 5.

As I have proposed before (Read, 1940, p. 250; this volume p. 41), I attach great significance to the ubiquitous presence of tourmaline in the crystalline schists. Boron is an atom of small radius and could be expected to travel far if influx is permitted in metamorphism. I recall some reasons for my belief. Barth (Balk and Barth 1936, p. 792) has described abundant tourmaline, often on the walls of shear-planes, in low-grade schists of Dutchess

County, and Turner (1933*a*, p. 182; 1934, p. 170; 1935*b*, p. 347; Turner and Hutton, 1941, p. 223) has attributed the abundant tourmaline in chlorite-grade greywackes of New Zealand to influx of boron material. Hutton (1940, p. 64) has given an excellent discussion of the significance of tourmaline in metamorphic rocks and has concluded that it is due to permeating boron vapours.

I have devoted the whole of an earlier Presidential Address (Read, 1940; this volume p. 1) to displaying the causal relationship between metamorphism and what I then called igneous action, so that I may be inclined to take this relationship as demonstrated. I have to record, therefore, that in this matter of tourmaline, for example, there is an opposite view: Goldschmidt and Peters have shown that boron is a widespread constituent of clayey sediments and Tilley (Williamson, 1935, discussion, p. 421) holds that this constituent provides the tourmaline of the metamorphic derivatives. On the other hand, McCallien (1934, p. 18) considers that metamorphic tourmaline has been derived from detrital tourmaline powder of the original sediment. The increase of tourmaline towards igneous or migmatitic centres shows that these alternatives are unlikely.

In the Dalradian Highlands and their prolongation into Ireland, tourmaline is found abundantly associated with albite in albite-schists, and this seems therefore a suitable occasion to link up the production of albite to that of tourmaline. A full discussion of the origin of the albite in the Dalradian albite-schists has been presented by D. L. Reynolds (1942). The discussion concerns impregnation with soda (Clough, Reynolds) or no transfer of soda (Cunningham Craig, Bailey, McCallien). Reynolds has shown by careful microscopic and chemical study that the albite is late and metasomatic: 'sodium silicate and P_2O_5 have been introduced into the biotite-schist to convert it into the albite-schist, and potash, iron, magnesia and possibly alumina, have been driven out' (Reynolds, 1942, p. 50). She correlates the production of albite-schists in the south-western part of the Dalradian area with the production of migmatites in the north-eastern part as both due to the emplacement of a syntectonic igneous mass along the axial belt of a recumbent anticline. These and other proposals of Miss Reynolds as they concern time can be left for next year, and all that I need do here is to reaffirm my belief that movement of material takes place in

all grades of metamorphism. This brings on for discussion the mechanisms of this movement, its different qualities and results. We have to study the processes known as metamorphic diffusion and metamorphic differentiation and the whole machinery of fronts. Let us begin with diffusion.

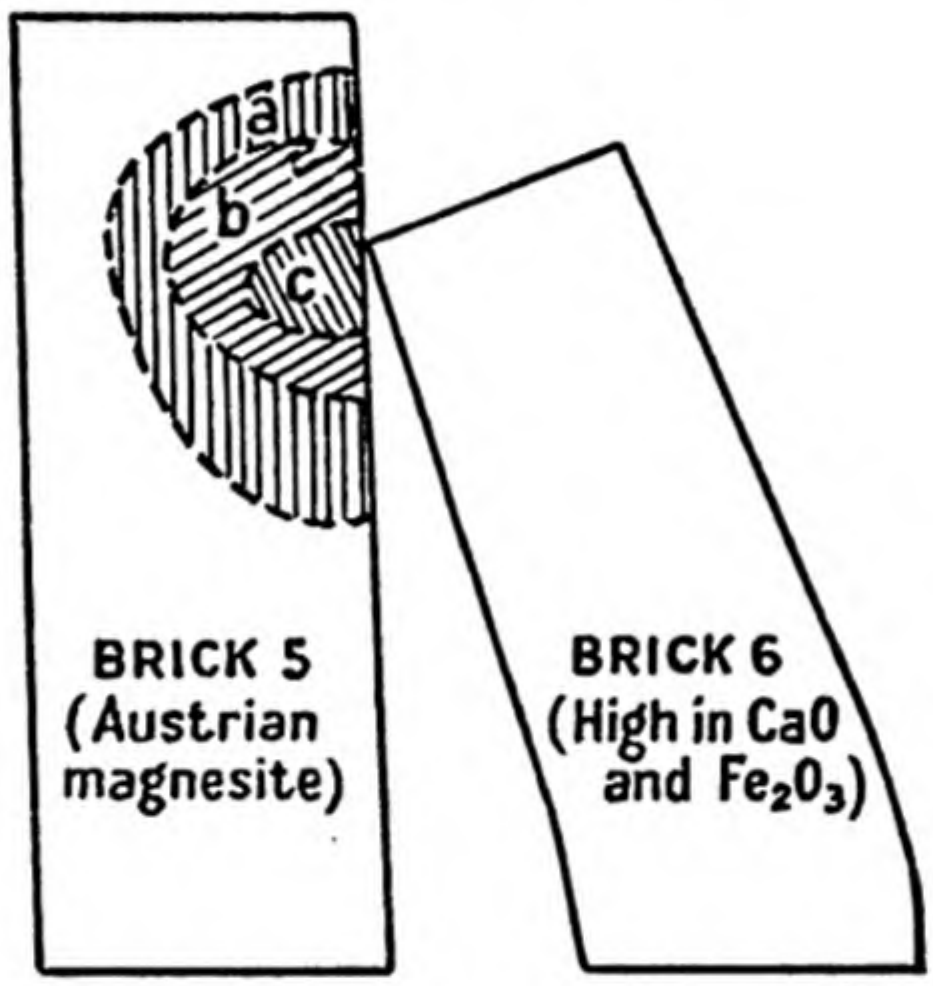
XII. DIFFUSION IN METAMORPHISM

Many authorities have decided that diffusion plays a very minor role in metamorphism. For example, Harker (1932, pp. 18–9, 75, 203–4, 300) considered it was so slow a process that the limit of effective diffusion was a small fraction of an inch. As evidence of this Harker pointed out that original narrow banding remained distinct; he did, however, admit that at high temperatures molecular diffusion could bring about a segregation into lenticles and patches of varying compositions. Niggli (Grubenmann and Niggli, 1924, p. 451) limited the action of diffusion in crystalline silicates to the boundary faces of the grains. Certain calculations of Hutton given by Turner (1941, p. 10) were held to indicate that in the production of conspicuous manganese-garnets in an Otago schist—the manganese content of the garnet and schist having been determined—there is necessary diffusion over no more than a milimetre.

Changes in solid rocks mean diffusion however limited in scope, and reactions at contacts of different kinds of solid rocks or of solid rock and magma mean diffusion. For example, Nockolds (1933) has pointed out that the retention of their outlines by xenoliths indicates molecular replacement by free diffusion, and Collins (1936) has emphasized that small inclusions, that should have been originally reasonably uniform, often are zoned, witnessing to diffusion into them of material from the enclosing magma. Contacts between different rocks, especially so different as gneiss and limestone, often show a mixed zone due to diffusion. Barth (Balk and Barth, 1936, p. 814) has described pelitic schist fragments enclosed in dolomite that are richer in magnesia and lime than the regional pelitic schists, indicating a 'calcium-magnesium metasomatism of the inclusions produced by emanations or solutions from the surrounding dolomite'. But, continues Barth (loc. cit.,

p. 827), 'Not only has there been a migration of atoms into the inclusions, but from the inclusions there has also been a stream of atoms into the limestone.' Weinschenk (1902) went so far as to interpret the peridotites and basic rocks at the famous granite-limestone contacts in the Pyrenees as due to diffusion streams. The transfusion process of Holmes (1936*a*; Holmes and Harwood 1937) is essentially one of diffusion. But the neatest case of

ADAMS' BRICKS



	SiO_2	Al_2O_3	Fe_2O_3	CaO
BRICK 6	1·0	1·0	22·4	8·4
ZONE c	0·6	0·5	17·2	4·0
ZONE b	1·6	1·3	15·6	5·4
ZONE a	3·5	1·7	12·4	6·3
BRICK 5	2·3	0·9	8·3	2·4

FIG. 6.

diffusion is that provided by Adams's bricks, especially as they give evidence concerning relative speeds of diffusion.

Adams (1930) studied two magnesite bricks that had come into knife-edge contact in a furnace. One brick, richer in lime and iron, had softened and bent over to touch the second brick; in this a change of composition had taken place away from the contact. By careful analysis, Adams established three concentric zones about the contact; the relevant data are given in Fig. 6.

It is clear that Al_2O_3, CaO, Fe_2O_3 and possibly SiO_2 passed across

the knife-edge contact into the second brick, and that lime has travelled farthest and iron-oxide the least. This latter result does not square with the relative atomic sizes of calcium and iron, but we can agree that if we were to make the two bricks touch along a plane instead of an edge, we could produce a brick with one half very much richer in silica, alumina and lime and the other half richer in iron oxide and magnesia. It becomes evident that the implications of Adams's bricks are not yet fully realized and it seems worth repeating the experiment. It is usually concluded that the transfer took place without the participation of water or mineralizers—that we are dealing with solid diffusion, but I am informed by Professor F. F. Osborne that a liquid phase was involved.

Jung (see Perrin and Roubault, 1939, p. 18) has described zones of different minerals around pieces of gneiss enclosed in limestone, and concludes that silica, alumina, iron oxides and magnesia become mobile under metamorphism and move unequal distances into the limestone, the relative order of increasing mobility being given as Fe, Ti, Al, Mg, Si. Jung proposes that mineralizers have facilitated these operations and that a two-way selective diffusion has taken place. Perrin and Roubault (1939, p. 18) regard such a differential transport of atoms in a pore-liquid as impossible, especially as temperature must be uniform over the short distances involved. There are accordingly two general alternatives before us, diffusion in the liquid or in the solid.

The importance of pore-liquid or intergranular fluid in metamorphism and other plutonic operations has been emphasized by Wegmann (e.g., 1935*b*, pp. 23, 25, 26, 38; 1938, p. 85; Wegmann and Kranck, 1931, pp. 58, 63). From his experience of well-exposed plutonic complexes, Wegmann came to the conclusion that the vast movement of material there required could be facilitated by diffusion processes through a small amount of intergranular liquid—the movement being controlled by gradients of various kinds, especially important being that of temperature. Eskola (1932*b*) has used the migration of pore-fluids to produce metamorphic differentiation. We see, however, at the present time, a greater emphasis being placed on diffusion in the solid rather than by an intergranular film. Perrin and Roubault (1937, p. 27), in addition to their criticism of Jung's proposals already noted, have

protested that the notion of pore-solutions is extremely vague, especially as to whether the liquids are indigenous or introduced and whether they displace themselves or merely permit an exchange of material—they compare the idea of pore-solutions to that of the ether, 'créée pour la commodité de l'esprit'. These criticisms do not appear to me to be very weighty—I should answer that they are both and can do both. But we must look into diffusion in the solid state.

There are plenty of proofs of solid diffusion in metallurgy and in mineralogy. We need only instance the homogenization of perthitic felspars at high temperatures and the sericitization of felspars or the experiences of cement manufacture. Some of the metallurgical examples are concerned with single crystals and conclusions drawn from them are not strictly valid for the multigranular aggregates of natural rocks, where surface processes are not automatically excluded. The mechanisms and controls of solid diffusion have been summarized especially by Perrin and Roubault (1937, 1939) and by Bugge (1946). It is considered that diffusion may take place either along the grain surfaces or through the crystal lattice. The atoms at the surfaces of crystals are unsaturated compared with those in the interior and are more easily moved and disordered. Many of the operations that go on in an intergranular film between surfaces cannot be much different from those of solid diffusion. In the migmatites and other plutonic rocks, these boundary effects are of very great importance (e.g. Cheng, 1944). Cracks and defects within the lattice and the common mosaic structure and disorientation of different portions of the lattice will facilitate boundary operations. Diffusion through the crystal lattice may be carried on in two ways. First, small ions may pass through the spaces between the atoms of open lattices; for example, the small Li and Na ions have been passed through thin quartz plates whilst the larger K ion has been retarded (Eitel, 1941, p. 115). Secondly, the ions may diffuse through the structure by jumping from one lattice position to the next one which is empty. Crystal lattices are imperfect edifices (see, for example, Lonsdale, 1947) and their imperfections both on a large and a small scale may be increased by distortion and shearing. Illustrations of these various forms of surface and inter-lattice migrations are available from the transfused quartzite xenoliths in the Colonsay

camptonite investigated by Reynolds (1936*a*, pp. 372–4; 1947*a*, pp. 220–1).

The chief controlling factors in diffusion in the solid are the following. An increase of temperature leads to an increase in diffusion—high-grade rocks are coarse. High pressures promote solid diffusion in that they decrease the pore space and lessen pore magma. Shearing stress distorts the crystal lattice and so promotes migration. Riecke's Principle applies whether liquid is present or not (cf. Goguel, 1943, p. 131). Grain-dimension is an important factor, as it is in any kind of diffusion—fineness of grain helps the reactions. As we have seen, the atomic radii of the migrating ions influence the relative speeds of diffusion. Obviously, the compositions of the reacting substances are of fundamental importance; the presence even in traces of an element foreign to a reaction increases the speed of migration and acts as a sort of catalyst, though, Perrin and Roubault conclude, the nature of grain contacts is often of much greater effect (1939, p. 51). And, of course, the chemical gradient is of vital significance. Bugge (1946, p. 57) has summarized the driving force of diffusion in the solid state as the *chemical potential* which varies with composition, external pressure, and temperature.

It is reasonable to hold that considerable amounts of material are moved in migmatization and granitization and it seems to me not unreasonable also to hold that the same applies to the great operations of regional metamorphism. Graton (1945, p. 148), for example, considers that 'Each of these far-reaching replacement operations [i.e. of granitization and pyrometasomatism] presumably requires a generous quantity of mobile medium (volatile agent) for the importation of the new mineral components and dissolution and exportation of those displaced.' But, on the other side, Perrin and Roubault believe that migmatites and metamorphic rocks are all due to diffusion in the solid (e.g. 1937, p. 33) and that such selective diffusion is the key to all problems of metamorphism (1939, p. 18). There is a tendency now for the exponents of granitization to divide themselves into *Wets* and *Drys*, according to their choice of soaking by fluids or of diffusion in the solid as the operative process. The *Drys* adopt, mistakenly I feel, that *either-or* attitude that I have deplored in the migma-magma controversy. Just as I believe that there are some

magmatic granites, so do I believe some metasomatic operations in plutonism go on in the wet as well as in the dry. There is no unique solution for the major problems of the plutonic rocks—we have to judge on the evidence presented by each case.

Before I proceed with the discussion, arising out of these remarks, of what I call channels of approach, it is fitting to consider the processes of metasomatism, and especially as they are presented in a recent synopsis by W. T. Holser (1947). A distinction must be drawn between transport and reaction, the former being controlled by the size of opening utilized. With supercapillary openings, there is unrestricted movement of fluids and large-scale replacement and deposition are possible—but for metasomatism there must be access to all parts of the host rock through smaller openings. In these capillary openings, 'diffusion of ions through a fluid may be more effective in transporting normally solid matter than mechanical transport by the fluid' (Holser, 1947, p. 395). Holser holds that some replacement is possible by use of capillary openings but that for more complete replacement still smaller openings are necessary. As we have already noted, most real crystals are mosaics bounded by submicroscopic cracks; along these grain-boundaries and through the inter-lattice openings, transportation must take place by diffusion through the solid phase and the reactions may not require any liquid to be present. Holser (loc. cit., p. 395) concludes that 'although the larger openings are important for fast transportation of large quantities of material to the vicinity of the replacement front, complete, fine-grained replacement may be accomplished only by transportation and reaction in much smaller openings'.

The dimension of the feeding channels is one control of the degree and style of metasomatism and therefore, as I believe, of regional metamorphism and plutonic processes in general. We see that there may thus be a free mobility in the higher levels of the crust in which fracturing is developed, whereas in the deeper parts diffusion may be inter-lattice or along grain-boundaries. Not only may there be this broad depth distinction, but in the deep levels a further ionic differentiation may arise in that the small ions may go through the lattice as well as around it, whereas the larger ions can only go around. The importance of solid diffusion will increase with depth—with increase of temperature and of

pressure. But to steer clear of any *either-or*, I have now to discuss the validity of channels of approach in plutonism.

XIII. PRIVILEGED PATHS

The channels of approach are privileged planes by virtue of structural, textural or compositional characters that permit an easier accession of fluids. Channels of approach may be a term that raises images of actual paths and canals, and it would be better to use privileged planes or, often more correctly, bands. The control of metamorphism by such planes is no new tenet. Van Hise (1898, p. 293) half a century ago invoked the action of solutions working along openings and surfaces of weakness and leading often to an emphasis of the bedding-planes of rocks. Grubenmann (1910, p. 65) considered that rocks of bedded or cleaved habit were metamorphosed more quickly than massive rocks because easy passages were offered to solvents.

Many of the privileged planes are the *s-planes* of Sander (1930, pp. 97–8), planes of weak cohesion, discontinuity and mechanical inhomogeneity, such as bedding, flow-cleavage, fracture-cleavage or an earlier schistosity. Cases of the utilization of *s*-planes by fluids in metamorphism are innumerable—we can only sample them here. Wegmann and Kranck (1931, pp. 62, 92–3; Wegmann, 1938, p. 52) have interpreted certain phenomena of migmatites as indicating transfer along the easiest paths, the solutions permeating along movement planes, for example, and spreading out from them; Billings (1937, p. 533) has shown how new platy minerals have formed parallel to the bedding 'because it was the most pronounced plane of weakness'; Chapman (1939, p. 176, pl. 5, Fig. 2) has illustrated garnet replacing strain-slip planes.

The prettiest examples of the utilization of *s*-planes are perhaps those provided by Balk and Barth (1936, pp. 714, 716, 789, 807, 809) from Dutchess County, New York. Balk (loc. cit., p. 714) concludes that, 'Doubtless, shear planes have been the chief avenues for circulating solutions during the anamorphism of the rock; the diffusion along these zones has been faster than in the matrix of the rock.' Biotite porphyroblasts are randomly oriented on the shear-planes, showing that no movement has taken place

since their growth. Barth ascribes the metamorphism of the rocks to soaking in 'hydrothermal solutions' which moved along shear-planes which have been the 'chief avenues'; these planes are studded with biotite, garnet, staurolite and kyanite. It should be noted that tourmaline occurs in an exactly similar fashion.

This utilization of privileged planes of the old *s*-type is the essence of the mimetic post-tectonic crystallization of the metamorphic rocks. Original platy minerals lying in the *s*-planes act as seeds and are enlarged, and new ones tend to grow with their greatest reticular density in the same planes. Mimetic crystallization may give rise to a well-oriented fabric by such utilizations of privileged planes. Friedel has remarked to Raguin (1946, pp. 63, 96) that the so-called force of crystallization seems to be a metaphysical notion that is used in an arbitrary fashion; restraint by the crystallization-space may be much more effective, and diffusion along specially easy directions may control the crystal shapes.

Privileged planes are usually of structural origin, but privileged bands depend on compositional and textural characters as well. If diffusion takes place, it is obvious that it must be controlled physically by the permeability and grain-size of a bed and chemically by its reactivity to the incoming fluids. The resistance to migmatization and granitization of extreme types of country-rocks such as limestones, quartzites and basic igneous rocks is an expression of this control (Read 1948*a*; this volume, p. 183) and need not be elaborated here. I consider now only a case of differential diffusion that has been given by Jamotte (1940, p. 1), who interprets certain relationships in the Musoshi complex as indicating that granitic mineralizers circulated especially in the coarser bands of arkose composition, and that only incidentally did they penetrate impervious pelitic rocks; permeability and composition control the advance of the emanations or of the granitic intrusion—this advance, concludes Jamotte, will accordingly be preferentially lit-par-lit. This famous expression we may now briefly examine.

XIV. LIT-PAR-LIT

In his classic memoir on the origin of the crystalline schists, Michel-Lévy (1887*a*) considered that the foliation of many gneisses

corresponded to an injection lit-par-lit parallel to the stratification. Gneisses resulted from a granitic metamorphism, one part of the minerals appearing by recrystallization, the other brought in by the magma (cf. Roques, 1941, pp. 8–9). For most purposes we have continued to follow Michel-Lévy in regarding lit-par-lit injection as a forceful intrusion of magma, but it is well to recall a rather neglected little paper of Edward Greenly (1903) in which not only was the formation of granitic gneisses by solid diffusion clearly enunciated, but also the mechanism of lit-par-lit injection ascribed to the same process. Greenly called attention to the extremely thin and regular seams of injected material as indicating operations in hot rocks and to the undisturbed film-like septa of country-rocks as affording 'some support for the view that the extension of the magma proceeded by quiet diffusion rather than by forcible injection' (loc. cit., p. 212). Barrell (1921), in his classic on the relation of igneous intrusion to regional metamorphism, affirmed that lit-par-lit structure resulted from the movement of solutions along schistosity planes; he finds evidence for capillary action in that 'the thinnest of parallel mica laminae may be traced throughout their length without showing such crumpling as even the inertia of the least viscous fluid would have given them if the invading magma had all been fluid' (loc. cit., p. 257). Fenner (1914) had earlier called attention to the undisturbed and unsupported nature of exceedingly thin septa in lit-par-lit complexes and had favoured a gradual transfusion rather than the violent intrusion usually pictured. Cheng (1944, p. 145) for certain Sutherland sheet-like bodies of granite suggests that they 'may be simply channels along which large amounts of magmatic fluids have repeatedly been introduced and have reacted with the country-rocks to form various kinds of granitic rocks'. There is a body of opinion, therefore, that favours diffusion rather than intrusion as the mechanism of lit-par-lit structure—but the manner of the diffusion remains a matter for discussion.

The advocates of solid diffusion, headed by Perrin and Roubault (1937, pp. 20, 21; 1939, p. 78) consider that lit-par-lit structure is a product of this style of diffusion. They point out that Michel-Lévy's view that gases promote the mobility necessary for lit-par-lit injection neglects the obvious fact that such injection must be at low temperatures and gas action must accordingly be small.

They hold that the great pressures in the depths are not favourable for the circulation, almost capillary, of magma between solid folia—a fact utilized every day in filter presses. They conclude that lit-par-lit injection is only a forerunner of granitization and not an end-product of solidification—it is a metamorphic process. We may keep the image of lit-par-lit but not its origin from granitic magma.

Perrin and Roubault (1939, p. 100) maintain that reactions in the solid are often propagated along privileged directions. The general experience of many field geologists indicates that channels of approach and privileged planes and bands have been utilized by fluids in the operations of metamorphism. When I contemplate the *s*-planes that may have been so utilized, I am faced with one of the fundamental problems of plutonism—the common coincidence of bedding and schistosity or foliation—a problem that I and many other metamorphic geologists have wrestled with (see, for example, Read, 1940; this volume, p. 1).

XV. SCHISTOSITY ON BEDDING

The coincidence of schistosity with bedding—especially over considerable regions of flat attitude—has given rise to the proposals of static or load metamorphism, advanced by Judd (1889), Milch (1894), Daly (1917) and others. A vertical pressure due to the weight of the super-incumbent load becomes the controlling factor; indeed, Milch divided regional metamorphism into two—load metamorphism under radial or vertical pressure and dynamic metamorphism under tangential pressure. Under the vertical pressure the original flat bedding becomes emphasized into the flat schistosity coincident with it (e.g. Grubenmann, 1910, pp. 103–4). I have dealt with this style of depth-control in previous pages and also on an earlier occasion (Read, 1940; this volume, pp. 15, 23, 26) and have sufficiently displayed the considerable difficulties in this proposal. I have therefore advanced the view that we are observing the result of the utilization of the *s*-planes of the bedding by solutions on metamorphism, and that consequently a great deal of the crystallization is mimetic. The privileged planes are the bedding-planes, and new minerals set themselves with

their planes of greatest reticular density in the bedding-planes. I need not repeat the arguments in favour of this interpretation—it will be more useful to consider some of the difficulties.

The first point concerns the general validity of interpreting the banding displayed by metamorphic rocks as bedding. Alternating layers of different compositions comparable with those of known sediments are considered to be bedding, and in many cases experience shows that this consideration must be correct. This does not, of course, invalidate the mimicry of any other *s*-planes such as cleavage, but I must emphasize that I am talking of bedding and not of bedding-planes, which raise a somewhat different question. If we compare the associations and transitions of many crystalline series of banded rocks with the associations and transitions of sedimentary series there can be no doubt that much metamorphic banding is developed on sedimentary bedding. We shall see that the operations of metamorphic differentiation often lead to a layered segregation; this segregation also is often parallel to what can reasonably be taken as bedding, as, for example, Billings (1945) has pointed out. Harker (1932, pp. 203–4) concludes that foliation and segregation are based ultimately on pre-existing structures, including stratification, folding and faulting on small scales.

But all banding in metamorphic rocks has not been interpreted as bedding. This is especially the case for the violent and conspicuous banding frequently displayed by the Archean. There is no reason why uniformitarianism should of necessity apply to Archean time, so that these propositions must be examined. Schmidt (1932, p. 182) has suggested that in banded gneisses an *Ortsregelung* has taken place controlled by the different capacities of translation of minerals under deformation: minerals with easier-gliding lattices or with most movable crystal shapes differentiate themselves from the less mobile minerals, so that from an originally uniform association of, say, quartz, felspar, garnet, mica, epidote and hornblende there might be produced light layers of quartz, felspar and garnet sharply delimited from dark layers of mica, epidote and hornblende. This concept of mechanical sorting into bands during deformation has been applied by Wenk (1937) to the beautifully banded gneisses of Ornö in Sweden. These rocks are banded hornblende-gneisses that show

hornblende of a constant composition throughout but have a significant variation in the nature of the plagioclase. The dark bands always have andesine, the light bands always oligoclase. Wenk concludes from petrofabric studies that a mechanical orientation and sorting of crystal grains according to their form and their relative capacity for gliding have produced the banding.

Bugge (1943, pp. 24–38) in his study of the Kongsberg-Bamble formation of Norway has discussed the general problem of banding in gneisses. This banding may be a primary igneous banding, a primary stratification, a lit-par-lit injection-product or the result of metamorphic differentiation of a homogeneous original rock. It is the last cause that we are now considering. 'By movements,' says Bugge, 'the minerals of the rock are set right and sorted regarding to their gliding capacity' and he interprets many of the Bamble banded gneisses as due to 'metamorphic differentiation in consequence of a mechanical deformation' (loc. cit., pp. 36–7). But Eskola (1939, p. 407) has remarked that the same result would be as well produced in the wet as the dry, and Turner (1941) has concluded that banding may be the result of a metamorphic differentiation brought about by chemical mobilization of the more readily soluble constituents of the rock—this chemical mobilization being especially active where mechanical action has already reduced the grain-size of the original rock—in other words, where a suitable *s*-plane or privileged band is provided.

But whether wet or dry, this *Ortsregelung* appears to me to be likely to destroy, or at least to modify, any original sedimentation or other primary textures. It is claimed (e.g. Read, 1940, p. 238; this volume, p. 23); Eskola, 1939, pp. 270, 276) that in dozens of cases minute original textures are preserved in completely recrystallized metamorphic rocks. Current bedding, graded bedding, varved bedding and similar small-scale original variations are recognizable. Surely *Ortsregelung* involving any considerable sorting out of newly-forming components is in these cases extremely unlikely. I prefer to believe that exceedingly often banding in the metamorphic rocks is bedding.

But banding cannot be bedding in all cases, nor can schistosity planes be bedding-planes. I refer, of course, to the innumerable examples of metamorphic rocks of igneous parentage which are completely schistose with a schistosity agreeing with that of the

associated metamorphosed sediments in which schistosity is on bedding. In whatever way we interpret schistosity, whether mimetic or dynamic, *s*-planes have to be produced in these rocks on their conversion to schistose derivatives. We cannot suppose that the whole of the necessary shearing or cleavage is taken by the igneous rock—the associated sediments must be affected as well. It is obvious, therefore, that in metamorphic series which have schistosity on bedding, the bedding-planes must have acted as slip-planes. It is unlikely that sedimentation textures are perfectly preserved in these cases and careful observations should reveal the imperfections. There is a delightful study available, for example, in the examination of the exact geometry of what we call current bedding in metamorphic rocks of various attitudes; I suggest that the relationships of denudation planes and deposition planes in the original current-bedded sediment would be found to be modified on metamorphism. Our use of sedimentation textures in many metamorphic rocks must be an approximation at best.

It is a matter of observation, as I have before remarked (Read, 1940; this volume, p. 23), that great areas of crystalline rocks, with schistosity on bedding, are horizontal or nearly so. This is the arrangement that has led to the invocation of static or load metamorphism because, as Daly (1917) felt, such an arrangement was 'truly inexplicable by pure dynamic metamorphism'. Becke (1912, p. 622) likewise found it difficult to explain widespread horizontal schistosity by tangential pressures. I have pointed out the equal inadequacy of load metamorphism to account for the arrangement. If there were present no schistose derivatives of originally massive rocks then we could be content with mimicry of an original flat-lying sedimentary series, but when such occur then we must consider whether the orthodox compressional view of metamorphism is really valid. I confess that in recent years I have become attracted to the idea of tangential movements activated by convection currents as the prime agent in the production of schistosity.

The *allure tangentielle* of different parts of the Massif Central has been emphasized in recent work of the French school. Thus Demay (1942) has interpreted the tectonics of portions of the Cevennes as due to tangential gliding and laminage of gneiss, often

over a magmatic substratum. Tangential movements and re-crystallization account for the 'allure tranquille souvent presque horizontale' (loc. cit., p. 17). Roques (1941), from his studies in the south-western part of the Massif, has drawn conclusions of great interest to plutonic geologists. He points out that the simple and most generally accepted idea that crystalline schists are the result of orogenic folding contemporaneous with rise in temperature cannot be true. If this idea were true, there should be a relationship between intensity of folding and development of schistosity—and this relationship is not found in the Massif Central. Orogenic forces are applied tangentially and produce folds; when schistosity appears it is of the slaty cleavage type, oriented perpendicular to the compression and often running obliquely to the stratification. But the schistosity of the crystalline schists, on the other hand, is always perfectly concordant with the stratification. There must be a distinction made between *tensions cristallophylliennes* and *tensions orogéniques classiques*. A horizontal schistosity requires either a vertical pressure or a horizontal stretching; vertical pressure can only be due to load and its increase with depth can correspond only to an increase of hydrostatic pressure. 'Il ne reste donc que la possibilité d'un étirement dans le sens horizontal' (loc. cit., p. 510). Roques's final conclusion is: in the play of forces which affect the earth's crust, the formation of crystalline schists represents the stretching, that of mountain-chains the compression, of deep layers.

Raguin (1946, pp. 101–4) agrees that metamorphism is not a direct consequence of orogenic deformation and favours Jung and Roques's invocation of stretching. But, Raguin decides, the only known geological phenomenon that can cause horizontal extension on a regional scale is the uprise (*l'ascension*) of batholithic bodies. This proposition, so far as ascension is involved, seems to me to be inadequate to explain horizontal schistosity. If I had to employ magma directly in this operation, I should prefer to use DeLury's (1941) method. DeLury suggests that the forceful migration of horizontal sheets of magma deforms the overlying carapace by differential frictional drag, with the development of a zone of horizontally schistose rocks with schistosity more or less parallel to bedding. The mention of frictional drag at once brings into mind frictional drag by convection currents.

Goguel (1943, pp. 97, 99, 109, 479–80) has briefly touched on the possible relation between convection currents and metamorphism in his treatise on the deformation of the crust. He concludes that metamorphism cannot be due to tectonic deformation nor, since it is developed over so great a volume, can it be due to a migmatization process. Elevation of temperature is clearly an effective cause but this, by itself, cannot account for a schistosity that is often horizontal or independent of stratigraphy. Pressure must be involved, and this must be horizontal tension since horizontal compression gives folding. Goguel associates the plutonic phenomena interesting us today with an exceptional efflux of heat and chemical agents from the earth's interior, activated by a horizontal tension in the crust. The totality of plutonic phenomena could be referred to a *bouffée* (puff) ascending in a convection current. Recently, Longwell (1945) has attributed certain extensive bedding-plane thrusts in the western United States to deep drag by convection currents; Longwell cites the relevant literature and makes mention of the contributions of Bull, Griggs, Hales, Holmes, Jeffreys and Pekeris. And still more recently, Vening Meinesz (1948), has given his great authority in support of convection currents in the plastic layer below the crust. I feel somewhat more comfortable in viewing horizontal schistosity, especially on bedding, as the result of frictional drag or extension rather than of orogenic compression.

XVI. METAMORPHIC DIFFERENTIATION

In what has gone before I have mentioned metamorphic differentiation in connexion with a variety of topics such as diffusion and banding. I wish now to discuss it in certain aspects in a little detail. It is entertaining and instructive to go back a century to Charles Darwin's work on the structures of metamorphic rocks in South America, as it deals with certain matters by no means yet settled. Darwin (1846) is concerned with three structures in the metamorphic rocks—cleavage, foliation and stratification. Cleavage is fissility in the rock, foliation results from the 'layers or plates of different mineralogical nature of which most metamorphic schists are composed', stratification means the 'alternate, parallel

large masses of different composition, which are themselves frequently either foliated or fissile, such as the alternating so-called strata of mica-slate, gneiss, glossy clay-slate and marble' (loc. cit., p. 141). Darwin disagreed with the view of Lyell and Sedgwick that foliation was the result of the metamorphism of layers of original different compositions, and held that foliation and cleavage were parts of the same process, in foliation there being 'a much more complete separation and crystallization' of the constituent minerals (loc. cit., p. 166). He observed cleavage cutting across true bedding, held that foliation was parallel to cleavage, and deduced that much of the so-called stratification of the crystalline schists, often coincident with foliation, could not be true bedding but was due to a process of segregation or concretion. He admitted, of course, that layers of marble and quartzite were true sedimentary beds. I believe that much of Darwin's difficulty was due to the inclusion of schistosity with cleavage, an inclusion still valid in America, for example; we see a legacy from it in this country where schistosity is usually considered to be a more advanced kind of cleavage and therefore automatically interpretable by the same mechanism. This extension of the cleavage process to include schistosity is, I feel, an error, but, however that may be, Darwin was not afraid to invoke movement of material and its segregation and concretion in metamorphism: that is, to invoke metamorphic differentiation.

Harker (1932, pp. 203–4, 300) has emphasized the necessity of distinguishing between foliation and schistosity and has followed Darwin in considering foliation as produced by a selective effect leading by stages of local solution, diffusion and recrystallization to segregation. These operations go on with increased activity in the higher grades of metamorphism or in injection metamorphism, and accordingly we find coarse foliated rocks produced. As I have already noted, Harker suggests that foliation is often controlled by some early textural character of the original rock. Barrell (1921, p. 256) likewise considers that gneissose foliation is more readily explained by movement of solutions along planes than by mere recrystallization in place.

The operation of regroupment of material during metamorphism is metamorphic differentiation. I have given a brief survey of this process with a number of examples linking the nature of the

metamorphic differentiates with their country-rock (Read, 1933, p. 324). Holmquist (1921) early stated the principles of this 'lateral secretion' in the production of the venites, and the whole matter has been explored by Eskola (1932*b*, 1939) and discussed by Turner (1941). Holmquist (1920, p. 209) appealed to a 'real fusion of the most fusible rock masses', a process developed by Eskola as 'differential anatexis'. Eskola (e.g. 1939) summarized the mechanisms of the movement of material within the rock itself as dependent on internal or external operations: internal either by the growth of crystals or crystal aggregates or by the concentration of the least soluble material, or by the extraction and reprecipitation of the most soluble material; external by the introduction or extraction of material and the general course of metasomatism. In his detailed studies of the Otago schists, Turner (especially 1941) has demonstrated metamorphic differentiation in his chlorite zone on the evidence that the same minerals are present but in differing amounts in all bands of the rocks, and their compositions are such as make them improbable as magmatic derivatives. Turner (1941, p. 14) suggests that granulation has aided componental movement, the pore-fluids being drawn into zones of granulation as the deformation proceeds, especially into the zones of strong mechanical movement. My object in detailing all these proposals concerning segregation and metamorphic differentiation is to lead to certain major difficulties in them.

Metamorphic differentiation is brought about by a segregation process apparently able to act against a chemical gradient. It has always seemed to me that the formation of a porphyroblast in a metamorphic rock was almost a miraculous event. The production of these differences in an originally uniform rock is most likely to be accounted for by Eskola's concretion principle—the piling up of an inactive mass by the rapid combination of migrating elements with elements of the host rock. But whatever may be the explanation on physico-chemical grounds, there can be no doubt on geological grounds that heterogeneity is very commonly produced in the metamorphic rocks. We have to realize, however, that in these days there is also a strident appeal to the opposite process—that of homogenization—in other plutonic rocks. Are plutonic geologists asking to have it both ways?

Homogenization is a process constantly appealed to by

transformists and granitizers in general. I have made the appeal myself. Goguel (1943, p. 477) suggests that homogenization is brought about by diffusion and Bugge (1946, p. 38) holds that in the arendalites and associated rocks, formed by reaction and diffusion in the solid state, 'there has taken place a homogenization of the heterogeneous rock series, the driving forces being the differences in chemical potentials'.

The homogeneity of any rock mass, be it sedimentary or volcanic or plutonic, is a problem of the first order. Bowen has found homogeneity a difficult operation in crystal-fractionation, the oil geologists know that no sediment is truly homogeneous, whilst the plutonists—as, for example, Perrin and Roubault (1939, pp. 120–2)—admit that their homogeneity is but relative. The degree of homogeneity in granitization depends on the nature of the country-rock being granitized, the temperature as affecting the velocities of diffusion, the nature of the diffusions required to granitize the varied country-rocks, and so forth. Niggli (1946, pp. 63–7) has recently discussed this homogenization in granitization and concludes that the chemical and mineralogical heterogeneity of the Finnish country-rocks is so extreme that by no system of anatexis and homogenization can the salic granite bodies be produced. He concludes that it is not a case of heterogeneous migmatites becoming transformed into homogeneous granite, but the reverse operation.

This is not the occasion to discuss these interesting matters. I have brought them forward so that in future discussions in plutonism they may be taken into account. Heterogenization and homogenization are not mutually exclusive operations, and the study of their controls will go deep into metamorphism. But we shall benefit by the simple recognition of the possibilities.

XVII. FRONTS

It is fitting that the last topic in these comments should be bound up with both place and time since it will form a connexion between these remarks and those of next year. The topic is that of *fronts*, a word that now takes its place beside the few such as 'assimilation', 'continental drift' and other explosive terms that

rouse dark passions in the most staid of geologists. As Bowen remarked during the Christmas festivities of the Geological Society of America at Ottawa last year, what is a front to one man, is an affront to another. The term itself seems to have arisen in geology to match its appearance on the political scene. Let us look into its history and significance.

At the 11th International Geological Congress at Stockholm in 1910, Pierre Termier (1912) continued the vigorous assault on dynamic metamorphism that he had begun at Vienna seven years before. Regional metamorphism was due primarily to the *colonnes filtrantes* coming from below and giving an afflux of elements from the depths that chased the old elements before them which, in their turn, displaced other elements. Wegmann and Kranck (1931) applied this mechanism in their detailed study of the Svecofennids. Wegmann proposed that around the *colonnes filtrantes* whose action produced the Hangö granite, there was a mantle with different kinds of enrichment in its different parts; in the inner part is an enrichment in SiKAl giving a potash granite, in the outer an enrichment in MgFeCa giving a cordierite-rich zone of kinzigites and related types. Using what I have called the 'pint-pot argument', Wegmann held that by introduction of material into a given space a whole series of migrations and chemical adjustments, made possible by the intergranular film, would be set in motion until they were stopped by falling temperature, small permeability and formation of insoluble compounds. In his great paper on migmatites, Wegmann (1935*a*) annunciated the doctrine of *fronts:* migmatization passes through the rocks leaving a gneiss and granite zone behind it. The changes take place at a kind of front, the relation between supply and cooling determining its position. When the reactable material in the host has been used up the front moves on if the gradients are favourable. If the front stands still a sharp contact results. Wegmann gave examples of fronts characterized by different materials, as, for example, the soda front of Stavanger, the potash front of Hangö and the magnesia front of Orijärvi. We may illustrate the mechanism by relating the formation of a magnesia front to an act of felspathization.

Wegmann (1935*a*, 1935*b*) distinguished between the phenomena of the upper non-migmatitic superstructure and of the migmatitic infrastructure lying below it. Between the two comes the seat of

regional metamorphism. During the pressing forward of the migmatite front the wave of regional metamorphism moves forward in sympathy. Wegmann also contrasts the migration of the migmatite front through stationary and moving hosts and its relation to episodes of granite formation—these are matters I have summarized before (Read, 1944, pp. 69–70; this volume, p. 127) and shall mayhap deal with next year. From Wegmann's proposals, it becomes clear that the migmatite front will often cut across the stratigraphy as also will the zones of regional metamorphism.

Backlund (1936, 1937) developed similar proposals; he related the spacing of fronts to the atomic radii of the participating elements and held that the migmatite front rises unequally. Primary *s*-planes were directional for the granitization. The unequal ascent of the front led to the variety of products—locally a homogeneous 'fundamental' gneiss was produced, in another place banded or veined gneisses, and in a third contact-like mineral associations. Metamorphic zones become swamped in the migmatites.

Allied interpretations have been widely applied in Scandinavia. Magnussun (1938) considers the Södermanland cordierite-gneisses as the result of a regional magnesium metasomatism connected with the intrusion of the Malingsbo granite. Barth (1938) has described a regional soda-metasomatism in the sparagmites of south Norway. Recently, Bugge (1943) has proposed that the rocks of Kongsberg-Bamble were 'hit by the migmatite front' and successively passed through the different metamorphic depth-zones.

The Fennoskandian doctrine of fronts has naturally not gone without criticism. Niggli (1946) has indeed questioned Wegmann's fundamental interpretation of the Hangö granite itself. If metasomatism has there taken place it must be, Niggli holds, exceedingly complete with much material moving in and out. He finds it difficult to believe in the kinzigite magnesia front that is complementary to the supposed granitization, since the rocks held to be modified by this front are not in the least degree richer in FeMgCa than were their original materials. There is in them an alkali transfer and Niggli maintains his old position that in Finland true granite magma produces migmatites at its margins.

However much the proposal of fronts may be criticized in

detail, I am certain it has come to stay. It is so useful a notion that it would be absurd to condemn it out of hand. With the progress in experimental work on metasomatism and with the growing realization that metasomatic processes are fundamental in metamorphism, fronts of one kind or another will continue to be invoked. For example, Holser (1947) in his consideration of metasomatic processes distinguishes between transport and replacement and envisages a replacement front with transport to and transport from and replacement at. Believing as I do in the unity of the plutonic series of granite-migmatite-metamorphite, I view the regionally metamorphosed rocks as resulting from the passage of waves or fronts of metasomatizing solutions out from the central granitization core about which arise zones of metamorphism (Read, 1940, p. 249; this volume, p. 40). Again, Holmes (Holmes and Harwood, 1937) regards the flux of emanations as a petrogenetic operation of great power, able to metamorphose, migmatize and even magmatize crustal rock material. The most modern developments on fronts, especially as concerned with granitization, are due to D. L. Reynolds (1943, 1944, 1946, 1947*a*, 1947*b*, 1947*c*), whose work we now examine.

We are immediately concerned with the *basic front*. In 1943, Reynolds described 'replacement' bodies of granodioritic rock rimmed by biotite-enriched hornfels zones and showed that NaCaSi had been introduced into the original hornfels to give the granodiorite, and AlFeMgCa removed to give the biotite-rich rims: these observations were elaborated in 1944, when she demonstrated the concentration of the outgoing basic material in the biotite-enriched contact aureole and certain ultrabasic roof-rocks. We have, therefore, a NaCa front in the main granodiorite and an MgFe front in the contacts. The basic front is become capable of producing igneous-looking basic and ultrabasic rocks; these basic complements of granitization are the *diabrochites* of Dunn (1942). What appear to be operations of the basic front on a grand scale have been described from America by Locke (1941) and Locke, Billingsley and Mayo (1940); the latter conclude that 'the problems of granitization would be concerned, not with what comes into the place, but with what goes out of it, and not so much with intrusion as with expulsion' (loc. cit., p. 533). What is possibly one of the best examples of the basic front has been described by

Johnson (1947) from the margin of the Idaho batholith where hornblendites and even peridotites are produced; Johnson has not interpreted these as basic fronts but as due to hydrothermal solutions coming from the granite. The results of Lapadu-Hargues appear to substantiate the operation of a basic front in regional metamorphism. I feel, as I have already said, that these results are an interim report needing a great deal of further work. I must admit that with regard to the 'basic front' variation of iron oxides and magnesia given by Lapadu-Hargues, it is necessary to point out that none of the metamorphic rocks contain as much of these oxides as the original non-metamorphic sediments, so that there can be no accumulation, on this evidence anyway, of these oxides in the outer zones of metamorphism.

We are not here to be concerned with these many fascinating problems of the basic front; I have said what I have said to restore confidence in Wegmann's original proposals. Whatever may be the truth with regard to transfer in the heterogeneous Finnish kinzigites, there can be no doubt about the nature of the transfer in such homogeneous sedimentogeneous rocks as those around the Newry granodiorite. We need a greatly increased body of chemical data to decide this matter. As Currier (1947) has recently said: 'there are few things that arouse the imaginative faculties of a geologist more than does a series of selected analyses'.

Reynolds (1947*b*, p. 211) has defined a front as follows: a front occurs wherever there is a diffusion limit marked by a change in the mineral assemblage. Whether we accept this or not, diffusion is held to be the main mechanism of front formation. Wegmann called in diffusion in the intergranular film, the most modern call in diffusion in the solid. I find two propositions connected with this diffusion limit not only instructive but also singularly entertaining. The first concerns the formation of rapakivi granite as this is seen by Backlund (1937, 1938*a*). According to him, exposures hitherto taken to indicate basal Jotnian arkoses resting unconformably on rapakivi granites really indicate the limit reached in the arkose by the migmatite front rising from below. The structure and texture of the arkose control the diffusion that is responsible for the granitization; the porosity of the host leads to easy permeation and the formation of homogeneous granite; bedding-planes and graded bedding of the arkose are pseudo-

morphed, as it were, in the mineral distribution in the granite; and, last but not least, specially crumbly patches in the rapakivi are due to 'an original variation in permeability so that certain patches of the arkose were, so to speak, "unwetted" by the emanations, and thus did not receive their final "cementing" material' (Read, 1944, p. 79; this volume, p. 143). These are very remarkable propositions, but somewhat similar and just as remarkable ones have recently been made by Perrin and Roubault (1941, 1945). If these workers are correct, then a sedimentary bed resting on a folded metamorphic rock, with discordance between the bedding of the sediment and the schistosity of the metamorphic rock, does not necessarily indicate an unconformity. The same appearance will be given if a layer of sediment acts as a barrier to the propagation of the metamorphic front. Since Perrin and Roubault believe that all diffusions involved in metamorphism take place in the solid, a loose open bed such as a conglomerate with a sandy matrix acts as an ideal barrier—the pores are too large for solid diffusion, as Holser would agree. Certain Triassic conglomerates hitherto regarded as unconformable on the underlying schists really show, according to Perrin and Roubault, a very rapid passage to the schists below, the metamorphic front having reached the conglomerate by solid diffusion and then quickly died in it—'est venu expirer dans celui-ci' (loc. cit., 1941, p. 185). Examination of the contact is held to demonstrate this transition and is reinforced by petrographical study.

These results of Backlund and of Perrin and Roubault have been accepted with some measure of acclamation by the most ardent of the transformists. Unhappily, I find myself unable to agree—this is not a matter of spiritual arrogance but rather of plain peasant caution. I cannot bring myself to regard migmatization or metamorphism as working in these ways—I propose to wait and see.

XVIII. THE UNITY OF PLUTONISM

During the course of these remarks, I have sampled the variety of relationships proposed between regional metamorphism on the one hand and igneous activity on the other—igneous activity here

including, of course, many operations that some of us think are not igneous at all. At one extreme is the view, say of Harker, that there is no causal connexion between the two processes; at the other is the view that there is the closest connexion. I have devoted the whole of an earlier presidential address and large parts of two others to the demonstration of the closeness of the connexion and have erected the Plutonic Rocks as a unified genetic group of metamorphic, migmatitic and granitic rocks as a testimony to my belief in the demonstration (Read, 1940, 1943, 1944; this volume, pp. 1, 44, 86). In between these extremes come a number of different opinions that are worth study; in that some of them involve time we may meet them again next year.

There is no need to repeat here the grounds for my belief in the plutonic group as a self-contained independent class of rocks. I refer here only to the earlier summaries that I have given in this present address of the work of such a diversity as Barrow, Vogt, the classic French, Balk and Barth, Cloos and Hietanen, Turner and Wegmann. To these could be added a score of field geologists who hold that metamorphism and so-called igneous activity are inseparably associated. Whether the whole of this company would agree with Currier's (1947, p. 77) recent proposal that granitization should be renamed invasive metamorphism, or with Wegmann's suggestion that migmatization is the propellant of the fronts that give regional metamorphism, or with my own invocation of plutonism does not really matter—they hold in common that regional metamorphism and 'igneous' activity are genetically related.

As I have already said, Horne and Greenly (1896) slightly loosen the bond between metamorphism and igneous activity, and this loosening becomes a distinct rupture in the classic work of Goldschmidt at Stavanger to which I have repeatedly referred. It will be recalled that Goldschmidt mapped at Trondhjem what may be considered the Barrovian zones of chlorite, biotite and garnet. In the Stavanger district he showed that there was superposed on the chlorite-zone rocks an injection metamorphism around intruded granitic bodies. By this injection metamorphism there arose a rather different series of zones culminating in migmatitic gneisses. These observations do not, of course, invalidate the concept of plutonism. The Barrovian zones are only one style

of the many styles of metamorphic zones or indeed of metamorphism—the style depends on the place-time position in the Granite Series (Read, 1948*a*), as I propose to display next year. In any case, Goldschmidt utilizes hot plutonic bodies to assist in the production of regional metamorphic zones in the South Norwegian Caledonian geosyncline.

We may now consider the modern French separation in place and time of the regional metamorphic rocks or ectinites and the migmatites. Demay's (1942) separation is slight; he holds that in the lower part of a structural edifice there is an interference with the tectonic happenings by magma or migma and the edifice may lose its basal parts by the uprise of magma or of the migmatite front. It seems to me difficult to ascertain how distinct are the operations of regional metamorphism and of migmatization from Demay's work. I conclude that the type of upward succession he would maintain would be: magma-granite, migma-granite, migmatite, permeation-gneiss, paragneiss, mica-schist, phyllites to slates. He considers that in the Cevennes it is not possible to recognize a deeper zone (that is, higher grade) of metamorphism than that indicated by biotite-paragneiss, independent of the influence of granitic impregnation (loc. cit., p. 130). From the point of view of zoning, it is of interest to note that sillimanite and cordierite are always associated with these granitic materials, sillimanite often being related to late alkaline injections. Demay (loc. cit., p. 131) concludes from the nature of enclaves in the migmatites and granites of the basal portion of his series that the initial metamorphism did not pass beyond a biotite-schist and biotite-gneiss grade, 'de zone assez haute'. I hope to return next year to the consideration of the important time aspects of Demay's Cevennes work. So far as place is concerned, we may note the variable height of the migmatite front in Demay's edifice—a conclusion leading to the more definite place-relations of Jung and Roques.

I have already presented a summary of the views of Jung and Roques on the 'stratigraphy' of the metamorphosed rocks—the ectinites—of the Massif Central and of their separation of ectinites (without felspathic transfer) from migmatites (with felspathic transfer). I now look into the zoning and formation of these migmatites, especially according to Roques (1941). The migmatites divide themselves into four zones from above downwards:

4. *Diadysites:* a zone in which the ectinites are traversed by abundant cross-cutting veins, *pénétration à travers*.

3. *Embrechites:* main zone of migmatites, made of felspathic impregnation-or permeation-gneiss, injection-gneiss, with augen and ribbon structures. The 'stratified' appearance of the ectinites is preserved, the micas being in continuous bands.

2. *Anatexites:* below the embrechites comes a zone in which the ectinite structure is confused and the texture is nebulitic, the micas appearing in patches and enclaves becoming abundant.

1. *Anatectic granite:* The ectinite texture is completely gone and the rocks are granitic.

The succession of these divisions may be regarded as a kind of 'stratigraphy' of the migmatites. But Roques has added a further 'stratigraphy' in these rocks of the sequence from above downwards of the three zones of muscovite, of sillimanite and of cordierite. Both these kinds of metamorphic 'stratigraphy' are superimposed on the succession of the ectinites (see Fig. 7). And besides, the superposition of the migmatite zones on the ectinite zones does not everywhere take the same pattern, for the migmatite front may rise to different levels in the ectinite sequence and reach anywhere from the bottom of the *gneiss inférieurs* up to the *micaschistes supérieurs* (Fig. 7). Let us look at a little of the detail of these superpositions, especially as regards the distribution of the key zonal indices.

First, in the Montagne Noire (loc. cit., pp. 55, 59, 74) the metamorphism is related to the emplacement of the migmatites except for the *micaschistes supérieurs* which are widely developed outside the migmatite area. This indicates that the migmatite front has reached to the level of the *micaschistes supérieurs* and that the lower members of the ectinite series have been swamped and transformed into diadysites, embrechites and, at the base, anatexites (Fig. 7, Type I). The upper part of the migmatites is characterized by andalusite and garnet, the lower by sillimanite and cordierite. Second, in the Rouergue (loc. cit., p. 137) the migmatite front mounts to the *micaschistes inférieurs*, the argument being analogous to that employed in the Montagne Noire, namely, the wide development of biotite typical of the *micaschistes inférieurs* outside the migmatite contacts (Fig. 7, Type II). Third, in the Bas-Limousin (loc. cit., p. 206) the mica-schist zones are above the

level of the migmatite front which attains the *gneiss supérieurs* horizon (Fig. 7, Type III); the *micaschistes inférieurs* contain such minerals as garnet, staurolite, kyanite and andalusite, the *gneiss*

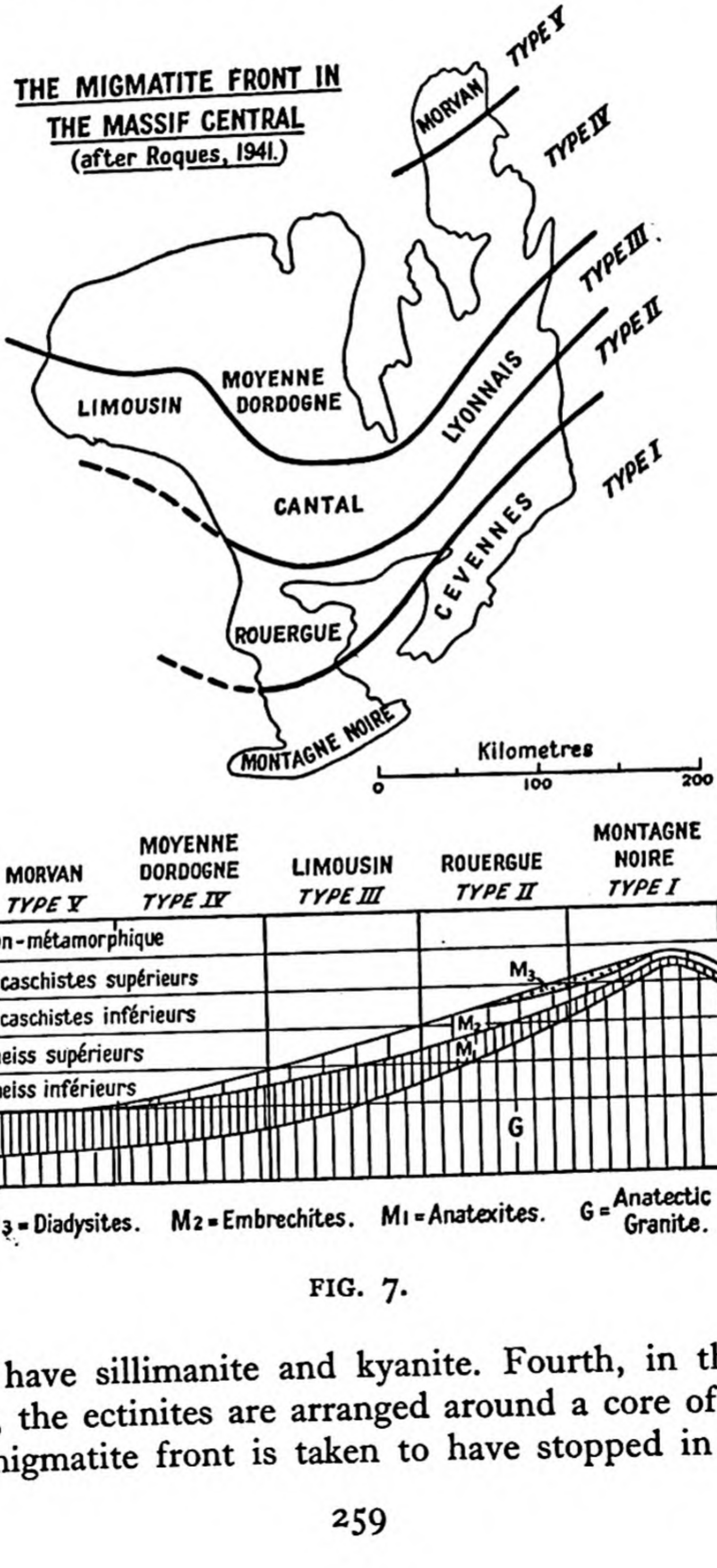


FIG. 7.

supérieurs have sillimanite and kyanite. Fourth, in the Middle Dordogne, the ectinites are arranged around a core of anatexite, and the migmatite front is taken to have stopped in the *gneiss*

inférieurs (Fig. 7, Type IV); it is to be noted that in this region, the migmatites are cordierite-bearing and the ectinites sillimanite-bearing. Finally, in the Morvan, the migmatite front is at the base of the *gneiss inférieurs* (Fig. 7, Type V). The position of the migmatite front in the different regions of the Massif Central is diagrammatically indicated in Fig. 7. The progressive ascension of the front from the north to the south is displayed.

This attitude can be extended from the Massif Central; in the Vosges, Jung (1936) places the front at the base of the *gneiss inférieurs*, whilst in the Pyrenees, Raguin (1938) considers that it rises specially high and comes into phyllites perhaps not much more than 3,000 metres from the surface. It appears, therefore, that there is a steady rise in the front from the Vosges to the Pyrenees. With one further statement we shall be equipped for the discussion of these remarkable propositions; the descriptions and diagrams of Roques show the complete independence of the migmatite front and the ectinite zones—'Celles-ci existent indépendamment des zones de migmatites' (Roques, 1941, p. 371). I propose to make a few comments now and most likely to return to the matter next year.

The division of the plutonic rocks into ectinites and migmatites in time and space is a proposition that requires consideration. I have already argued that transfer takes place in all plutonic rocks. Lapadu-Hargues sees no need for the two divisions, since no break is indicated by his chemical study of the plutonic series. To confine our attention for a moment to the ectinites, we note the two 'stratigraphical' groups, schists above and gneisses below. Roques draws attention to these two ectinite facies, the first without visible felspar, the second with felspar macroscopically evident, but maintains there is no appreciable difference in composition between the two. I consider this to be exceedingly unlikely, and apparently Raguin (1946) does the same for, whilst accepting the ectinite zones, he calls upon a migration of alkalis in depth on a grand scale. It is clear that a great deal more chemical work is required on the lines of Lapadu-Hargues, but I propose to regard the schist-gneiss place-sequence over great areas as indicating transfer on a regional scale. If the ectinite-gneisses are not due to transfer, then the 'stratigraphy' of the Massif Central becomes very remarkable indeed.

Most of us believe we can distinguish between a schist and a migmatitic gneiss, but how does the field-worker tell an ectinitic gneiss from a migmatitic gneiss? Personally, I should find this a difficult operation, and apparently this difficulty has also been experienced by Jung and Roques. The *gneiss supérieurs* should be reasonably easy to distinguish from the migmatitic gneiss—but Jung and Roques (1938, p. 126) find the dividing surface not at all apparent, since it separates gneiss from gneiss, but they are still able to determine the boundary 'avec toute la précision désirable' (loc. cit., p. 143). In the *gneiss inférieurs* the surface becomes more obscure, but the line can still be traced on a map, 'en tenant compte d'un certain facteur d'appréciation personnel' (loc. cit., p. 126). With the personal factor coming in, then we may expect a variety of interpretations. Roques (1941, p. 225) finds it difficult on occasion to tell the origin of the rocks, and Raguin (1946) admits a convergence in depth of the *gneiss inférieurs* and the migmatites. The personal factor is again revealed by Raguin (1938) in his description of his Pyrenees work; the zone of diady-sites is recognized by the occurrence of two veins at two localities and several others at two other localities. I doubt whether my personal factor would permit me to state that rocks with sillimanite, staurolite, cordierite are still phyllitic (loc. cit., p. 19).

All workers in the migmatite complexes would agree on the rock-types that make the four zones of the Roques-Jung series, but not all would accept them as of stratigraphical value. It is of interest to note that when we look at the zones resulting from the superposition of the migmatite on the ectinite zones we obtain a general picture like that of almost any region of metamorphic rocks. It is important, therefore, to consider the Jung-Roques proposals for a time-separation between ectinites and migmatites, though this is a consideration that will be more appropriate in a year's time. Though the migmatization is broadly contemporaneous with the general metamorphism (e.g. Roques, 1941, pp. 332, 352, 367, 502), it occurred during the later stages of the metamorphic period, the ectinite zones existing before the rise of the migmatite front. The ectinites are not related to orogeny but to geosynclinal deposition and depression in the course of which the sediments have been subjected to increased temperature and pressure and to stretching. The rise of the migmatite front and its

very accessibility depend upon orogenic happenings during which the ectinite and migmatite zones may become inverted. It is from this viewpoint that we can agree with Marcel Bertrand's dictum that 'chaque chaîne de montagnes a ses gneiss'—for only in such folded geosynclinal piles can the universal and general metamorphism be revealed to our observations and only there does the migmatite front rise into the ectinite series.

It is clear that these interesting propositions, like many others I have mentioned during the course of these remarks, involve the dimension of time as well as of place—we shall accordingly meet them again next year. The time–place relationships between the granitic and migmatitic portions of the plutonic rocks on the one hand and the metamorphic portion on the other—relationships that Jung and Roques and almost everyone I have mentioned in these pages have been concerned with—are enclosed to my comfort, at least, in the Granite Series (Read, 1948*a*; this volume, p. 192). This series attempts to relate the plutonic phenomena at the various levels of exposure and to give a unity to the processes of granitization, migmatization and metamorphism at depth and at successively higher positions and at later times. We examine it in the next address.

In the beginning of this address, I asked for the plutonic rocks to be made to live, to become cinema films. Goguel has said 'la kinématique n'est que de la géométrie dans le temps'. The purpose of these remarks is to direct your attention to the geometry of the plutonic rocks, their stratigraphical elements, schistosity, foliation, zones and fronts, so that we can apply time to this geometry and reveal the history of no mean portion of the Earth.

VI

A CONTEMPLATION OF TIME IN PLUTONISM

1949

I. TIME AND TIME AGAIN

AMONG the many profound observations of that distinguished potomologist, Mark Twain, is one that states that very few things happen at the right time, and that the rest do not happen at all—it is added that the conscientious historian will correct these defects. Maybe, as these proceedings advance, my conscience will come into action along these lines, but the attentive critic will surely recognize the event. Nevertheless, at any moment of exaltation, I should be humble to remember the sage of the Mississippi, for my purpose is to add time as a fourth dimension to the three-dimensional place of last year's inquiry into plutonic happenings (Read, 1948*b*; this volume, p. 194). We have to endeavour to see how far we can watch the wheels go round—this is always a pleasant occupation, but one made still more pleasant on this occasion by the variety of the wheels and the diversities of their speeds, for time in plutonism seems to have many qualities. We have to begin by surveying some of their general aspects.

A first concerns length of time. We often find a refuge in the immensity of geological time, but this refuge is not always secure. For example, uniformitarianism cannot be eternally true and, when we know a great deal more about plutonism, we shall have to discuss whether the time-dimension has changed with time and whether distinctions based on age are to come back into petrology. Again, we are given to quoting from the classics—Hesiod, Hero-

dotus and the rest—that if we had enough time we could do anything. This can be only a half-truth. An inadequate force cannot become adequate by virtue of time—it is unlikely that, however long I lean against the Bank of England, I shall unaided bring it down in ruins. We have therefore to distinguish between an eternally inadequate force and one that appears inadequate over our short period of observation. Though diffusion, for example, may be slow it may still be able to produce gigantic results given time. The speed of plutonic processes such as metamorphism and granitization is one of their fundamental qualities and has to be integrated with the speed of tectonic and other mechanical operations going on in the crust.

A second aspect concerns what start and what end shall be given to the time of development of a plutonic rock and how shall this be divided. All rocks, because their elemental materials are all of the same age, are complex documents, but they may record either countless episodes of a long history or only the very last chapter. An equilibrium-rock, for example, witnesses to the one final act in its production and two equilibrium-rocks that might have had profoundly different earlier histories may by metamorphic convergence (Read, 1937*b*) come to be essentially alike in that they record a common concluding act. The abrupt temporal limitations of geological maps must be early realized in plutonic studies; space they record but rarely time, and they assume too readily that every rock is an equilibrium-rock capable of being related to one or other of a small number of episodes in the history of that part of the earth's crust. We shall begin to advance in the interpretation of such terrains as the Lewisian and Dalradian of Scotland, for example, when we represent time as well as place on the geological map, when we turn the map from a still into a film.

The choice of the limits of a plutonic history may be difficult to make. We must free ourselves, I feel, from the compartmentalism of the stratigraphical column and be prepared, if need be, to envisage a plutonic story requiring the half and even the whole of geological time for its telling. When we see displayed on a geological map a granite labelled Upper Carboniferous, for example, we must realize that we are being told only the date of the death of that particular plutonic rock. Death, I agree, is an important event to a

man, but his life is much more important to posterity—and so it is with the plutonic rocks. Besides, just as all members of a family do not usually die all on the same day, so genetically related granites may complete their courses at widely different dates; we ought not to shy from discussing the possibility of a granite labelled Devonian by the field-geologist belonging to the same genetic series as one labelled Carboniferous or Tertiary and, indeed, of all granites belonging to one series. The notion of eruptivity has brought with it the notion of a sudden happening and many geologists have been influenced thereby; it is important to remember that eruptivity refers to place—the eruptive rocks appear suddenly to the geologist as the geological map is being made, but they themselves have been emplaced as the final act of a very long history.

As it is with the granitic portion of the plutonic rocks, so it is with the metamorphic portion. We cannot date a metamorphism, we can only date the end of what is usually a long and involved process, not capable of being unravelled by the study of equilibrium-rocks but of petrographic relics and geological records. On occasion we may be lucky enough to observe the birth-time of a metamorphic rock in a sedimentary series, but often we can make only an arbitrary choice and it is our custom to denote the state of the rock at that given time as original. The history of a plutonic rock is never finished; we have to inquire how far it can be broken up into chapters before the appearance of the rock at the surface of the earth. But we must remember that a major tectonic event recorded with violence in the stratigraphical column does not automatically terminate a plutonic history. Into these high themes I enter later on, but in this general survey there still remain a number of plutonic time-aspects to be introduced.

The contrast is often made between the prolonged sequence of crystallization of the components of the igneous rocks and the simultaneous crystallization of the crystalline schists as these differences are revealed in their textures. But this simultaneity, if it is applicable at all, can apply only to the equilibrium-rocks which, fortunately, are relatively rare and imperfect in development. At the present time there is a danger in some schools of paying overmuch attention to the classificatory virtues of the metamorphic facies concept. We have to keep the equilibrium-rocks

in their proper perspective and give the greater heed to the non-equilibrium portions of the plutonic rocks since these reveal earlier episodes in the metamorphic history. Metamorphic facies, by itself, will not get us far; it is only the study of relict minerals and textures that can make the rock live. One of the fundamental branches of plutonic geology, therefore, is concerned with textures as indicating the time-relations of the rock components. Certain textural relations are capable of clear and unambiguous interpretation, others are not. Slowly a body of experimental and geological evidence is being built up that may resolve some of the present ambiguities.

But whilst I may seem to have frowned upon the glorification of metamorphic facies *per se*, I must admit that this may yet prove to be of great time-significance in the study of plutonic history. If we could be sure that, possibly over a somewhat restricted field, a metamorphic facies was peculiar to a given time in the development of the metamorphic rocks of that field, then the plutonic geologist would have gained a principle as valuable to him as William Smith's Second Law is to the stratigrapher. There are certain limited advances already clear to me. Thus, it seems reasonable to propose that identical metamorphic facies in the same ground are isochronous as well as isophysical. This proposition is, of course, the core of metamorphic convergence and is one, I feel, that lies at the heart of the solution of many gigantic problems of the plutonic rocks, notably, for example, that of the origin of the granulites and granite-gneisses of the old massifs of central Europe. We must, clearly, be as cautious as we are able in this matter and, in view of the variety of factors controlling metamorphism, not be unduly astonished at metamorphic irregularities. In any event, the correlation of metamorphic facies must not be construed as indicating a stratigraphical correlation, an admonition particularly applicable in the study of the Scottish Highlands.

In the consideration of plutonic time, a number of dates have to be distinguished and kept distinct when possible. First is the age of the original material, sedimentary or otherwise, that has subsequently become metamorphosed, migmatized or granitized; but debates on this topic, in that they are largely an affair of comparative stratigraphy, must be kept apart from the discussion of the more purely plutonic ages. There is general agreement—though,

so far as I remember, no one has given any cogent reasons for the opinion—that granitization and metamorphism are very slow processes requiring great spans of time for their accomplishment. It is required, therefore, to determine the geological dates of the beginning and the ending of the metamorphism or other plutonic activity, and in certain cases this has been found possible. But plutonism does not appear to be a steady process advancing at a uniform rate, but rather one of energetic pulses interrupted by operations of diverse kinds, especially those of deformation. One of the greatest fields of inquiry in plutonism is concerned with the time-relations of crystallization and deformation or, as the learned say, of crystalloblastesis and clastesis. Here, again, certain cases are clear: deformation has followed crystallization or crystallization has concluded the metamorphic action, and so forth. But here also doubts arise, especially in the cases where movement and crystallization may be synchronous or where mimetic crystallization may have operated.

Though it be admitted that plutonic processes revealed in the rocks require great time, and maybe all time, for their accomplishment, there still arises an associated inquiry of major importance. This considers the question whether all the phases, pulses or episodes seen to have operated in a plutonic rock constitute one cycle or many independent cycles. We are here really scrutinizing the validity and limits of polymetamorphism. The French maintain (e.g. Lapadu-Hargues, 1948, p. 142; Roques, 1941, p. 24) that metamorphism is the normal and permanent phenomenon under certain thermodynamic conditions probably related to crustal depth. The conditions of metamorphism can be changed, in particular, by dynamic action which places the elements undergoing metamorphism in another thermodynamic envelope. The evolution ends in the metamorphic state that the rocks now present to us. Plutonic rocks, therefore, might supply evidence of their sojourn in a succession of different thermodynamic envelopes; they would be styled polymetamorphic and we would interpret them as having undergone several metamorphisms. As I have already said, the appearance of the rock at the surface of the earth should terminate at least a chapter in its history, but yet this chapter may not be the last in that volume. This great problem as to whether plutonic history is one unit, several unrelated units

or a series of episodes can only be attacked by a number of small assaults. We may, however, obtain a clue for final success by regarding the prime cycle: formation of migma, formation of magma, movement of magma, consolidation, upheaval and denudation.

In the foregoing, I have outlined some of the general aspects of time in plutonism. I have now to discuss in such detail as is permitted certain features of this survey, but before this is done, a decision has to be made on two matters. First, I repeat once again my opinion that the plutonic granitic rocks are not near blood-relations of the volcanic basaltic rocks. I propose to assume that this opinion is by now commonly accepted by all reasonable men. Secondly, I repeat once again my opinion that the constant use of progressive in the description of metamorphism and other plutonic processes is in many cases unwarranted and misleading. This second opinion may seem trivial beside the first, but it is, to my mind, of fundamental importance in the interpretation of plutonic history. Metamorphism, for example, is presented to us so often as a sequence of progressive changes that we tend to lose sight of the fact that the sequence may be only one of place and not of time. Graphic pictures of the metamorphic or migmatitic front passing through the crust, or of the depression of a geo-synclinal filling into greater and greater depths with the successive production of zones of increasing metamorphism are often specious simplifications. The preservation of original sedimentary textures, for example, in the high-grade rocks of a so-called progressive series and their obliteration in the low-grade rocks—a fact about which there can be no question—indicate that the observed series is not one of time; the high-grade rocks did not pass through the low-grade stage. The demonstration of a time-sequence depends on detailed textural studies, as we shall see later. Moreover, whilst we admit progression in metamorphism, we must not thereby exclude regression; we must be prepared to consider the proposal that even the highest-grade rocks are re-gressive from a grade still higher but not exposed to us in the earth's present crust. With these two decisions made, we can pass to more specific considerations.

II. TIME AND CRYSTALLIZATION

We have seen that the plutonic class of rocks encloses the genetic series of granitic, migmatitic and metamorphic portions which in some measure result from an accentuation of a steady process. Whilst this may be so, the records of the process may be very differently displayed in the three portions. In the granitic rocks, for example, crystallization may be the last dominant act and may blot out or at least blur the evidence of earlier history; on the other hand, metamorphic rocks often reveal a dozen stages in their development. The common massive character of the granites, contrasted with the schistose or foliated character of the metamorphites, expresses this same diversity. But this diversity is more apparent than real as it arises from the temporal and local balance between the two controlling processes of plutonism—the chemical and the mechanical. In the first process, the rock becomes chemically reconstructed into a mineral assemblage in accord with the operative temperature, pressure and concentration; in the second, it is subject to deformation depending in type again upon a physical and chemical environment. For convenience, we may call the first process crystallization and the second deformation.

Crystallization and deformation are rarely simultaneous—even when they are said to be so, they seldom begin and end at the same time but usually overlap. Crystallization can be entirely earlier or later than deformation, or the two processes can vary in dominance during the same plutonic event and so make a pattern of phases in the proceedings. Either process may be so overwhelming that a special character is given to the products, as is seen in dislocation metamorphism and contact metamorphism. It is clear, therefore, that even if the consideration of those detached and mobile portions of the plutonic rocks that are frequently called igneous is postponed, there still remains for unravelling a sufficiently complex set of time-relations in the autochthonous products such as granitization-granites, migmatites and metamorphites. It will be convenient to begin this task with cases in which the crystallization factor is dominantly involved and then, later, to bring deformation into the time-picture. We are therefore now to be

concerned largely with the sequence of crystallization in the classic Rosenbusch sense, or with the time-differences between minerals as revealed in the *in situ* products of plutonism.

We have first of all to agree that there are time-differences in the constituents of the plutonic rocks, though this leads to rejection of the dictum of our master Becke (1904, p. 563; 1913, pp. 4, 35), who, with his over-emphasis on equilibrium, saw the metamorphic constituents as of simultaneous formation. If simultaneous means what I take it to mean, then the examination of the textures of almost any plutonic rock—and even of the equilibrium types of them—will show that crystallization has not been simultaneous for all components but is a diversified and lengthy sequence of events. In this study of textures we can derive great help from metallurgy and ore-microscopy, but, if we accept this help, we must remember several cautions. The chief are these: a given texture, as for example the graphic, may arise in several ways; the experimental conditions of metallurgy are different from the natural conditions of plutonism; the lattices of plutonic minerals are silicate and not metal or sulphide lattices, and plutonic rocks are multigrain and multicrystal aggregates.

In textural studies dealing with sequence of formation, we are concerned mainly with three inquiries—simultaneous crystallization, either absolute or approximate, replacement and veining. Simultaneous crystallization is exceedingly difficult to establish from an examination of textures. Even an apparent exsolution association may be misleading, since one of the components may be oriented relics in the later replacement (cf. Cheng, 1942, p. 80, Fig. 5). Smooth and regular contacts between minerals—the mutual boundaries of the ore-microscopist—are often put forward as evidence of simultaneity, but such boundaries have been produced in artificial replacements and are shown by pairs that can be demonstrated by other criteria to be of different ages. Again, intimate and intricate intergrowths do not prove simultaneity, as can be illustrated in many partial pseudomorphs of limonite after pyrite and in the sericitization of felspars. Graphic intergrowths are often loosely taken to be the result of simultaneous crystallization. These intergrowths may look like eutectics but that does not make them eutectics, and essentially similar textures are produced by a variety of operations such as

replacement, unmixing and recrystallization of an aggregate. There is no proved eutectic in the plutonic rocks nor, from the nature of the case, is any to be expected.

The erection of criteria of replacement has occupied ore-microscopists since their special study began (e.g. Colony, 1928; Newhouse, 1928; Bastin and others, 1931; Harvey, 1931; Lindgren, 1933; Edwards, 1947). Unlike simultaneous crystallization, replacement can often be proved beyond reasonable doubt but several criteria may be required to establish the case. Surface relations of crystal units have perforce to be used and a three-dimensional check is generally lacking. The host controls replacement and some host-character must persist into the new guest; replacement may be guided by a cleavage or by some crystallographic direction in the host-mineral, or by a texture such as bedding or slaty cleavage in the host-rock. The form or the texture of the host may be partly or entirely pseudomorphed by the guest, the contacts between them being jagged and irregular—the caries contact—, and islands of host with similar orientation to that of a mainland left in several dissimilarly oriented guest-grains. These criteria are excellent and may often be reinforced by those provided by veinlets, since veining is likely to be of replacing character. Veinlets of irregular width, with walls that do not match, and cutting across several diversely oriented host-grains are of the greatest significance, but veining must yet be used with care. Threads running between crystal grains or incipient tongues inserting themselves into a mineral are not to be trusted. Further, replacement may be selective, so that it is possible for a vein to be earlier than its present walls, its original walls having been replaced; veins can be pre-wall or post-wall.

Even with all these chances of error, it is possible to demonstrate a sequence, absolute or overlapping, in the crystallization of the constituents of the plutonic rocks—examples are provided immediately. For the most part, we are dealing with a sequence exhibited by different minerals, but on occasion it is necessary to determine the crystallization history of a monomineralic rock such as a quartzite or limestone. Here it is reasonable to use a variation in grain-size since, in any one event, the form-energy of the grains would be approximately equal over the limits of a thin section. Thus, in the siliceous Moine granulites of the Tongue

area in Sutherland, the normal regional grain-size of quartz is much reduced along certain planes and greatly increased along certain paths—a circumstance I interpret as indicating two different kinds of event affecting the original granulite.

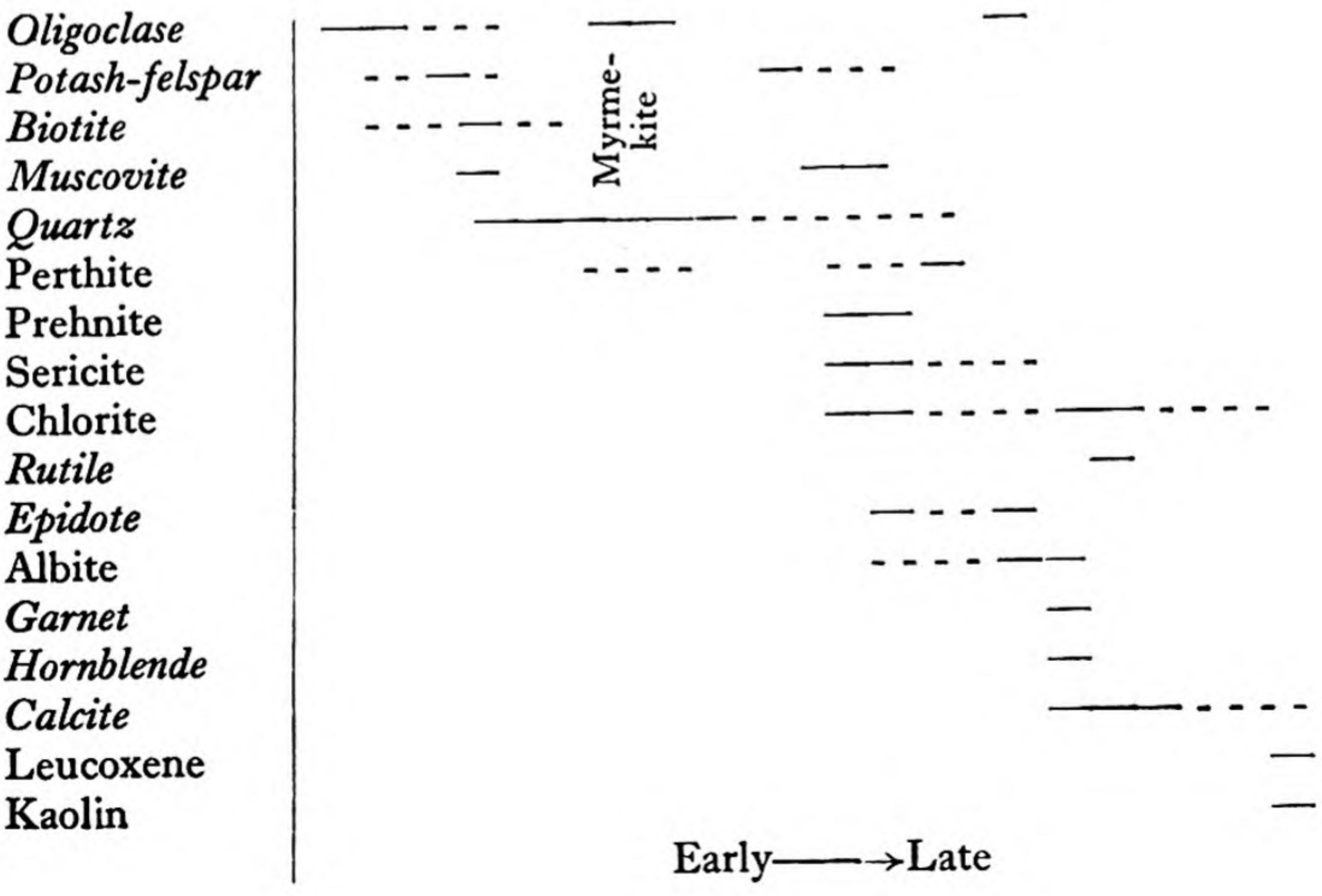
In time-studies of the kind we are considering, it can often be inferred that the sequence of mineral formation belongs to a unified episode—there is a reasonable progression in the crystallization and often it is apparent that a later mineral is the consequence of an earlier reaction; this is particularly well shown in many migmatites. But sometimes it is likely that the sequence is divisible into episodes separated by considerable stretches of time. In this case, the geological history of the region is of fundamental significance; it would be just as unwise to study the Alps or the Highlands by thin sections alone as it would be to study them without thin sections. In this broader time-differentiation, relics of earlier crystallizations, as of earlier fabrics, are of course of the greatest interest.

I now give a selection of examples, some from my own experience, that illustrate the determination of sequence of crystallization in the plutonic rocks and, at the same time, the complexity of this sequence. I begin with a few simple cases. In Unst in the Shetlands, pelitic rocks of two tectonic units show, in the one, replacement of kyanite, staurolite and garnet by chloritoid and chlorite and, in the other, replacement of andalusite by kyanite—along the tectonic boundary metamorphic convergence has occurred with the disappearance of these minerals and the production of a convergent sericite-chlorite-schist (see Fig. 11); three terms in a time-sequence have thus been determined definitely in the field and under the microscope—what has not been determined are the intervals between the terms (Read, 1934*a*, 1937*b*). Billings (1941, p. 921) has dissected three major stages in the paragenesis of the Littleton formation of Mount Washington, New Hampshire, marked by first andalusite, followed by sillimanite, staurolite, garnet, tourmaline, muscovite and biotite, and concluded by chlorite and sericite. Again, in the kinzigites of the Austrian Waldviertel, Waldmann (1927, p. 67) finds kyanite replaced by granular and fibrous aggregates of sillimanite with the retention of external form. In the Forbes Range of New Zealand, Turner (1934, p. 163) has described albite-quartz-epidote-chlorite-phyllites that show

large irregular relict grains of hornblende, quartz, plagioclase and ilmenite; two episodes are evident in the history of the rock. A different criterion is used by the same worker (Turner, 1933*a*, p. 226) to give results on another scale of time; in a second New Zealand schist the plentiful biotite of the rock is never included in the large albites present, from which it is inferred that the biotite is wholly later than the albite—the inference is of a certain validity and other considerations would determine how great a time-separation was implied. Coming back to Scotland, I may note Janet Watson's (1948) demonstration that the formation of fibrolite is later than felspathization in the Kildonan migmatites—here the veining criterion is employed. In the Bettyhill migmatites farther north, Cheng (1942, 1944) has established from textural studies an elaborate paragenesis that is worth presentation (Table A) as an example of complex crystallization history. In this table, minerals shown in italics are found in the pre-existing rocks and part of them were recrystallized or formed at the same time as the

TABLE A

CHENG'S PARAGENETIC SEQUENCE IN THE BETTYHILL MIGMATITES



oligoclase. Cheng concludes that here is the record of the repeated activity of various fluids which at the beginning of the operations were able to produce large-scale replacement of the constituents but at the end were confined to minor actions along grain-boundaries and fractures. Du Reitz (1938, p. 27) has presented the early portion of Cheng's time-scale in the Muruhatten migmatites of Sweden. Perrin and Roubault (1939, pp. 67, 163–8) interpret the textures of many granites as resulting from a superposition of incomplete transformations and crystallizations, one crystal growing at the expense of another.

The examples just provided group themselves into two types—those, as Cheng's, where the operations revealed clearly belong to one episode and those, as mine from Unst, where this on geological grounds is unlikely. There is often a difference, too, in the criteria used—in some cases, the relationship between two crystals is the decisive observation, in others the relationships are more widely textural. Excellent combinations of both styles of evidence are provided in the work of E. Cloos and Anna Hietanen (1941) on the 'Martic Overthrust' region of Pennsylvania and Maryland.

TABLE B

SEQUENCE OF CRYSTALLIZATION IN THE WISSAHICKON SCHIST

(Anna Hietanen, 1941)

Albite, Quartz I, Pyrite I
Muscovite I, Chloritoid, Chlorite I
Biotite I, Phlogopite I
Tourmaline I
Garnet, Microcline
Biotite II, Phlogopite II, Pyrite II
Muscovite II, Quartz II
Dolomite
Calcite
↓ Tourmaline II

In the Wissahickon Schist and associated rocks, Hietanen (loc. cit., p. 114) has determined the sequence of crystallization given in Table B on microscopical and field evidence. As an example of the methods, we may present the interpretation by Hietanen

(loc. cit., p. 149 and Fig. 45, p. 144) of a muscovite-chlorite-schist with albite porphyroblasts, belonging to the Octoraro Schist. A thin slice of the rock is given in Fig. 8 based upon

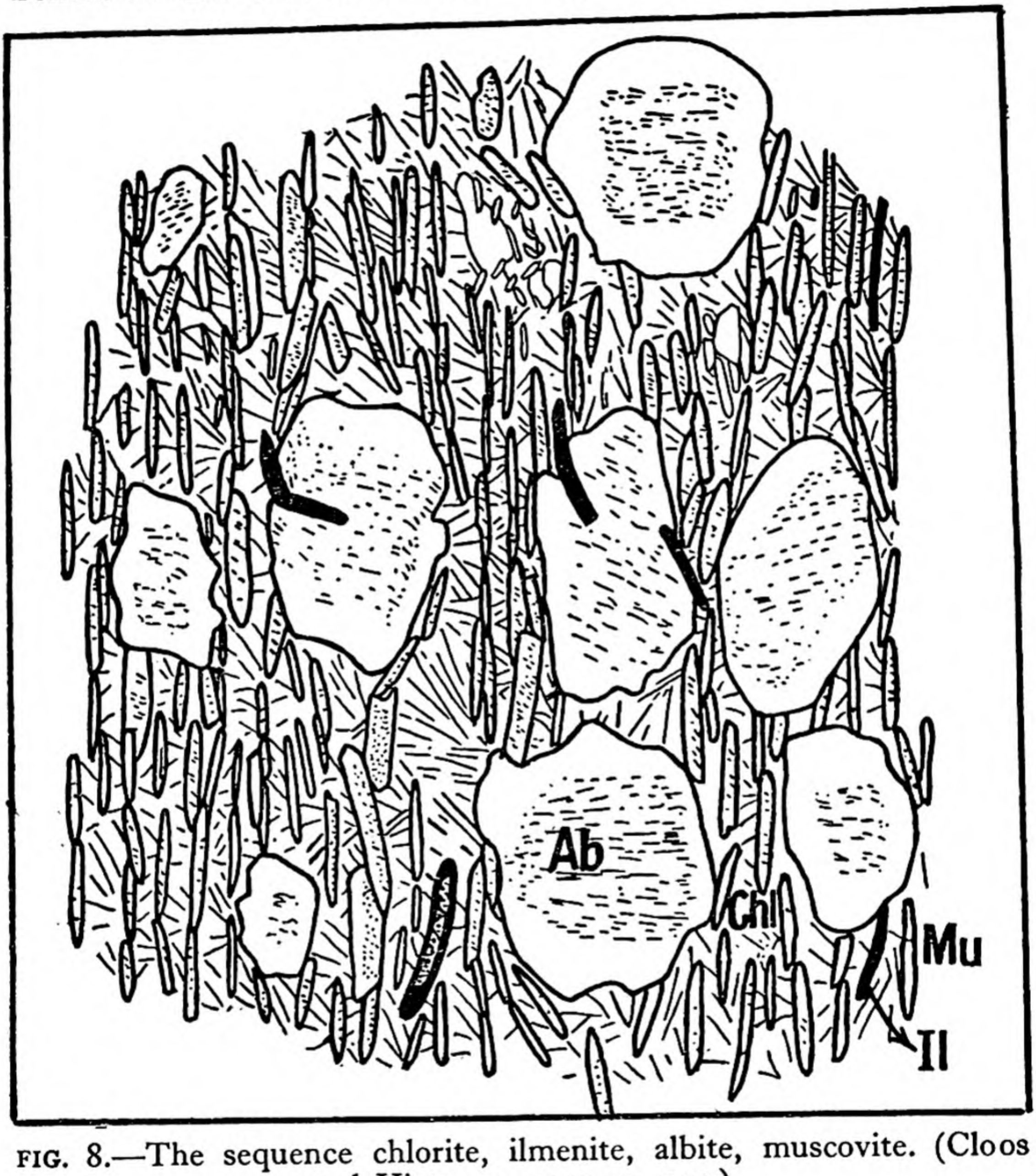


FIG. 8.—The sequence chlorite, ilmenite, albite, muscovite. (Cloos and Hietanen, 1941, p. 144.)

Hietanen's Fig. 45. The dissection, so far as the minerals visible in the figure are concerned, is as follows: an early cleavage is indicated by the orientation of small muscovite and chlorite flakes, and by the trends of the inclusions in albites and rows of dust particles in large micas. These large micas are late muscovite

considered to have recrystallized parallel to shear-zones, so that a later cleavage has been superposed on the earlier one. The albite porphyroblasts are later than the oldest structure preserved by the trends, and the large muscovites are later than the albites because their orientation is undisturbed around them. Ilmenite scales are included in the albite and are older. The sequence of the minerals concerned is thus: chlorite and early muscovite, then ilmenite, then albite and lastly muscovite again—an order perhaps not immediately acceptable by all on the microscope evidence presented, but one based on principles that Turner, as has been noted above, takes as valid.

In this section of these remarks, I have endeavoured with but partial success to be concerned with only sequence of crystallization, but deformation will break through. I postponed continuing Waldmann's history of his aluminium silicates, otherwise we would have seen that the sillimanite, after it had been formed from kyanite, was distorted into knot-shaped patches or rolled out into fine streaks that plastered themselves on large grains of other minerals (Waldmann, 1927, p. 67). It is evident, too, that the Octoraro Schist just dealt with records a sequence of deformations as well as of crystallizations. We have, therefore, to take up the more complex questions of the time-relations of crystallization and deformation.

III. TIME, CRYSTALLIZATION AND DEFORMATION

In the first few pages of his welcome memoir on the evolution of the metamorphic rocks, F. J. Turner (1948, pp. 10, 11) expresses the general opinion that the fabric of a metamorphic rock—that is, the sum total of its textural features as revealed by the mutual relationships between crystalline aggregates, between crystals of the aggregates and between groups of ions and even ions of the crystals—is a record of the intensity and kind of deformation involved in the making of the rock. Most regionally metamorphosed rocks bear the imprint of broadly contemporaneous deformation, and schistosity can usually be correlated on petrofabric evidence with deformation (loc. cit., p. 11). In his earlier, 'essentially non-genetic' definition of schistosity, Turner

(1936, p. 202) considered that this type of fabric was 'determined by crystallization or mechanical deformation of the rock-forming material in the solid state under the influence of stress or high temperature'. The group of rocks the fabric of which is related to movement of the textural elements, and which is called tectonites by Sander (1930, p. 62), includes the *secondary tectonites*, the common metamorphic rocks that display a fabric resulting from differential movements of constituent grains during deformation. Though we may not all be competent to master the intricacies of the petrofabric dissections of *Gefügekunde*, we can at least admit that slipping on one or more sets of planes, flexural slipping, rotation of textural elements and other propositions involving deformation appear as reasonable interpretations of fabric. Even mimetic crystallization, taking place after all deformation must have ceased, may be controlled by a deformation that provided certain privileged paths for solutions or privileged planes for a growth-fabric (cf. Read, 1948*b*, p. 186; this volume, p. 239). Deformation, therefore, has an importance not only in its own right but, especially today, in its time-relations to other plutonic processes.

The most important textural character of the crystalline schists is the preferred orientation of their mineral components, an orientation either of crystal-form or of crystal-lattice. The form-orientation is manifest in schistosity, but the less obvious lattice-orientation may be of as great a genetic significance. As already noted, preferred orientation can be produced either during or after deformation; in the first case, by mechanical movements such as rotation, gliding, translation or rupture of existing grains, or by chemical operations involving growth of new minerals or the adjustment of existing minerals under the conditions imposed by the deforming forces; in the second case, by mimetic crystallization on privileged planes. The greatest contribution of the Sander-Schmidt school of petrofabrics has been its emphasis on the movements influencing fabric rather than on the forces responsible for the movements. Sander's *Bewegungsbild* is a picture, based on the fabric data, of the movements involved in the making of the rock. *Bewegungsbild* can be translated as movement-picture; we could just as well use motion-picture instead, since Sander's viewpoint is in essence kinematic. Time and the sequence of events have come again into the study of the metamorphic rocks.

I have to confess that a great part of the complexities of modern petrofabrics is beyond my comprehension. I make this confession the more readily when I recall the findings of Ernst Cloos and Anna Hietanen (1941, pp. 9, 37, 177) in their massive petrofabric investigation of the 'Martic Overthrust' region that the 'same sequence of crystallization has been determined microscopically and confirmed by petrofabric analysis' (loc. cit., p. 177) and that the fabric agrees with the megascopic structures (loc. cit., p. 9). I am comforted to find that in most cases 'it seems a waste of time to use the universal stage and statistical analysis' (loc. cit., p. 37). But, all the same, even I can appreciate the many time-results from Cloos and Hietanen's study, especially as embodied in their correlation between the sequence of crystallization and the nature of the fabric diagrams (e.g. loc. cit., Tables 20, 21). The earliest minerals are in very complete girdles, the latest accumulate in point-maxima. Cloos (loc. cit., p. 187) concludes that mineral orientation was controlled by several successive shear-planes; the first cleavage-planes that were formed were used by the oldest micas, planes developed later were used by later micas, and between these stages a southward rotation of the successive planes took place.

Unsuspected time-intervals of a large order may become revealed by petrofabric studies. Sahama (1936) has shown that in the Lapland granulites two deformations are recorded, an earlier by the megascopic fabric of planar structure and lineation and a later by the preferred orientation of the quartz; as E. B. Knopf (1938, p. 72) puts it, 'upon the earlier movement there was stamped an oblique overprint of a later deformation now recorded in the quartz fabric'. Being well informed concerning my limitations as a petrofabric expert, I do not propose to proceed farther with this contemplation but to dwell more upon time-differences that are recognizable by the working petrological geologist.

Still confining our attention to events reasonably taken to be connected portions of a longer and self-contained episode, we may examine one of the most attractive fabric features of the metamorphic rocks—the evidence for rolling or rotation of porphyroblasts as exhibited, for example, by snowball-garnets, twisted chloritoids or by albite trends.

Many porphyroblastic minerals show a multitude of small

inclusions of groundmass minerals that are arranged in parallel trails, curved or straight. Often these trails are emphasized by a conspicuous but inert component, such as graphitic dust, that is not wanted by the porphyroblast. In most cases, the trails or trends, as the Americans call them, can with confidence be taken as relics of an earlier parallel texture, an *S*-plane such as bedding, cleavage or schistosity. The relative attitudes of such textures inside (internal *s*, *si*) and outside (external *s*, *se*) the porphyroblasts are clearly of significance for the history of the rock. Before I proceed to discuss this significance, however, I have to make two points of definition. First, if we propose to use the term *helicitic*, we had best do so in the same sense as Weinschenk, as indicating a texture presented by inclusions, the trails of which are relics of a folding earlier than the crystallization of the porphyroblast—helicitic has a time-significance that, if the word is used at all, should be preserved. Second, to employ the term trend, as Ingerson (1938) has done, for inclusions that are controlled in position by the lattice of the porphyroblast and are clearly not relict but of very late production, is to confuse the issue. Ingerson's trends raise quite a different problem, that of the *gefüllte Feldspäte*, which have a time-significance of their own (e.g. Read, 1937*b*; Cornelius, 1935). With these two points accepted, we can examine some pretty examples of relict textures and discuss their implications.

Over half a century ago, that master of detail C. T. Clough (1897, pp. 40, 43, 76) observed what he took to be true helicitic texture in the albite porphyroblasts of the Cowal albite-gneisses (see Fig. 9),—'the puckered foliation planes of the rock being traceable through the albites by the help of frequent inclusions' and 'the movements being earlier in age than the albites' (loc. cit., p. 40). Clough considered that it was 'not certain, however, that they [the albites] are later than all these movements' since 'a discordance in direction has been brought about between the foliation preserved in them and the foliation now seen at their sides' (loc. cit., p. 43). He concluded (loc. cit., p. 43) that 'the albites may have received a twist'. This twist must have been of considerable magnitude, since Clough records divergences of 90° or so. Rotation of the Cowal albites during their history is clear from Pl. vi. of the *Geology of Cowal*, and Bailey (1923) has since

interpreted this plate as showing that the mineral was alive and actively growing during the rotation. This is possibly the case, but the freedom from inclusions of the marginal parts of the albites in some occurrences indicates that further incidents have to be written into their history. These Cowal albites serve to introduce us to the study of trends.

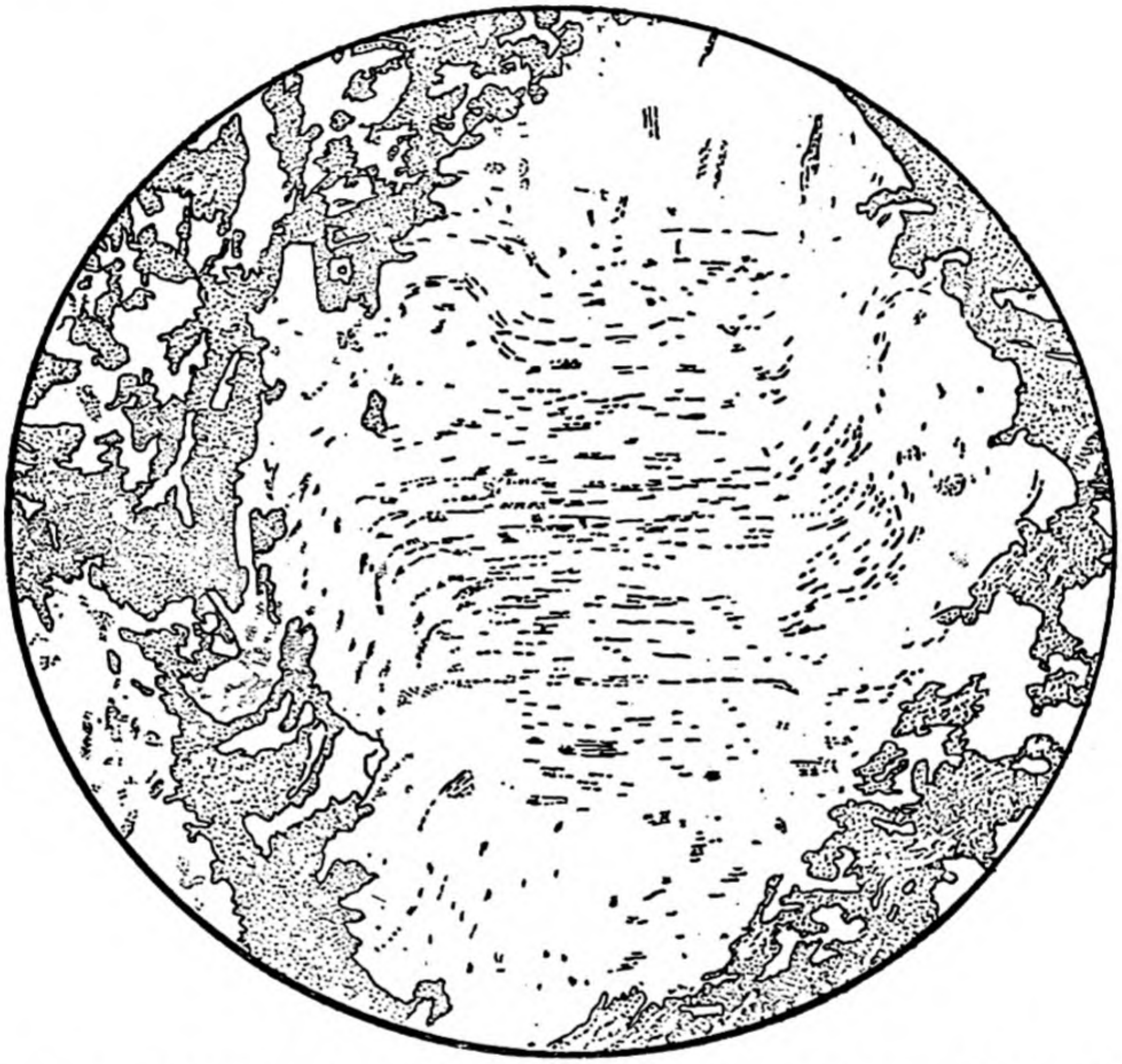


FIG. 9.—Trends in albite. (After Teall, Geology of Cowal, *Mem. Geol. Surv. Scotland*, 1897, Pl. vi.)

In his early and classical investigation of the chloritoid-schists of the St. Gotthard border, Niggli (1912) analysed the 'folding' shown by relict trends enclosed in porphyroblasts. This folding could be of three different origins: first, the result of the conservation of an already folded groundmass; second, the result of simultaneous folding and crystallization or the natural rotation of the plane of maximum tangential strain during the metamorphism; and third, the result of the later rotational strain in crystals

already formed. In the modern jargon, we would now label these three types of folding as pre-crystalline, para-crystalline and post-crystalline respectively—we elaborate these terms later.

Returning to trends in albite, we find an abundance of observations and deductions. To begin with, for the Scottish South-West Highland albite-gneisses, Cunningham-Craig (1904) held that the albites were later than the movements that folded the trends, agreeing in general with Clough's interpretation of the adjacent Cowal examples; we have already seen that Bailey (1923) has shown that these examples exhibit para-crystalline deformation. In the albite-gneisses of Otago, Turner and Hutton (Turner 1933*a*, pp. 229–30; Turner and Hutton, 1941, p. 233; Hutton, 1940, p. 33) have used the attitude of trends to decipher a complex history. In certain cases from western Otago, curved trends indicate para-crystalline movement, but elsewhere there is a complex folding of the trends of epidote and iron oxide inclusions visible within the limits of a single crystal—an indication of pre-albite deformation; but it is often observed that at the boundaries of adjacent porphyroblasts the trend may change abruptly by as much as 90°—an indication of external rotation of the porphyroblasts subsequent to their crystallization. The interpretation of the albite fabric is clearly first, strong deformation with the formation of the *si* now enclosed in the albite, second, growth of porphyroblastic albite under static conditions, and lastly, a late deformation involving rotation of the porphyroblasts with their enclosed *si*. The interpretation of trends in the albite porphyroblasts of the Wissahickon Schist of the 'Martic Overthrust' region of Pennsylvania and Maryland is complicated by the presence of multitudes of inclusions parallel to crystallographic planes of the albites. Hietanen's opinion is nevertheless clear: 'Albite recrystallized in an early stage during the movements which continued after it was stabilized. Some albite, however, may belong to a later phase (post-deformation crystallization).' (Cloos and Hietanen, 1941, pp. 134–5, pl. 18). Turner (1948, p. 151) has given the general rule for the interpretation of *S*-shaped trends in porphyroblasts: trends shaped like an ordinary *S* indicate an anti-clockwise rotation of the porphyroblast, like a reversed *S* a clockwise rotation.

Provided that a large area of a slice or, preferably, of a selected rock-surface is examined and that care is taken that the trends are

being viewed in the plane at right angles to any axes of rotation, then many entertaining investigations of detail appear likely. For example, work along these lines on specimens, oriented on the ground, of the albite-gneisses of the South-West Highlands or of Ireland would reveal the direction of movement during the albite incident—and this alone—in the metamorphic history of the relevant parts of those regions. It is possible, too, that refinements of observation might lead to substages in the albite history being deciphered, similar to those determinable in certain rolled garnets discussed immediately. Further, I know of no cases of spiral trends in albite—the rotation appears to be of limited amount; it is to be expected, therefore, that an investigation of the amounts of rotation undergone by all the albite porphyroblasts in a certain space, and the correlation of these amounts with the attitudes of the porphyroblasts to the schistosity, would build up a convincing movement-picture for the albite incident.

Though trends in albite may be good, trends in garnet may be very much better. To introduce garnet trends, we cannot do better than begin with our magnificent native examples of snowball-garnets described and interpreted long ago by Flett (1912, Pl. ix, Fig. 6, and p. 111). In the garnets of a stippled schist from Carn Chuinneag in Ross-shire, Flett figured inclusions arranged in a spiral—an arrangement he interpreted as showing that 'the garnet was rotating under the impulses received from streams of material flowing with unequal velocities past its two sides. It was being rolled along, and was growing larger, like a snowball, during the process.' Movement and crystallization were clearly simultaneous; the garnet, in the academic phrase, was para-crystalline, in the vulgar phrase, a snowball-garnet. A sketch of Flett's garnet is given in Fig. 10.

A few years later, Krige (1916; see also Mügge, 1930) in his petrographical account of the Bündnerschiefer on the southern margin of the St. Gotthard massif, described many examples of trends in garnet and staurolite and discussed the time-significance of these textures and of others shown by his rocks; Krige's conclusions are well worth examining in a little detail. He distinguishes garnets of two ages; early shapeless crystals crowded with inclusions with *S*-trends and clearly rotated, and late, solid well-shaped crystals that push aside the groundmass minerals

or enclose them in helicitic fashion—late plagioclase behaves in the same way as this late garnet (loc. cit., pp. 580, 594, 606, 610, 622). In some rocks, staurolite shows no *S*-shaped trends whereas

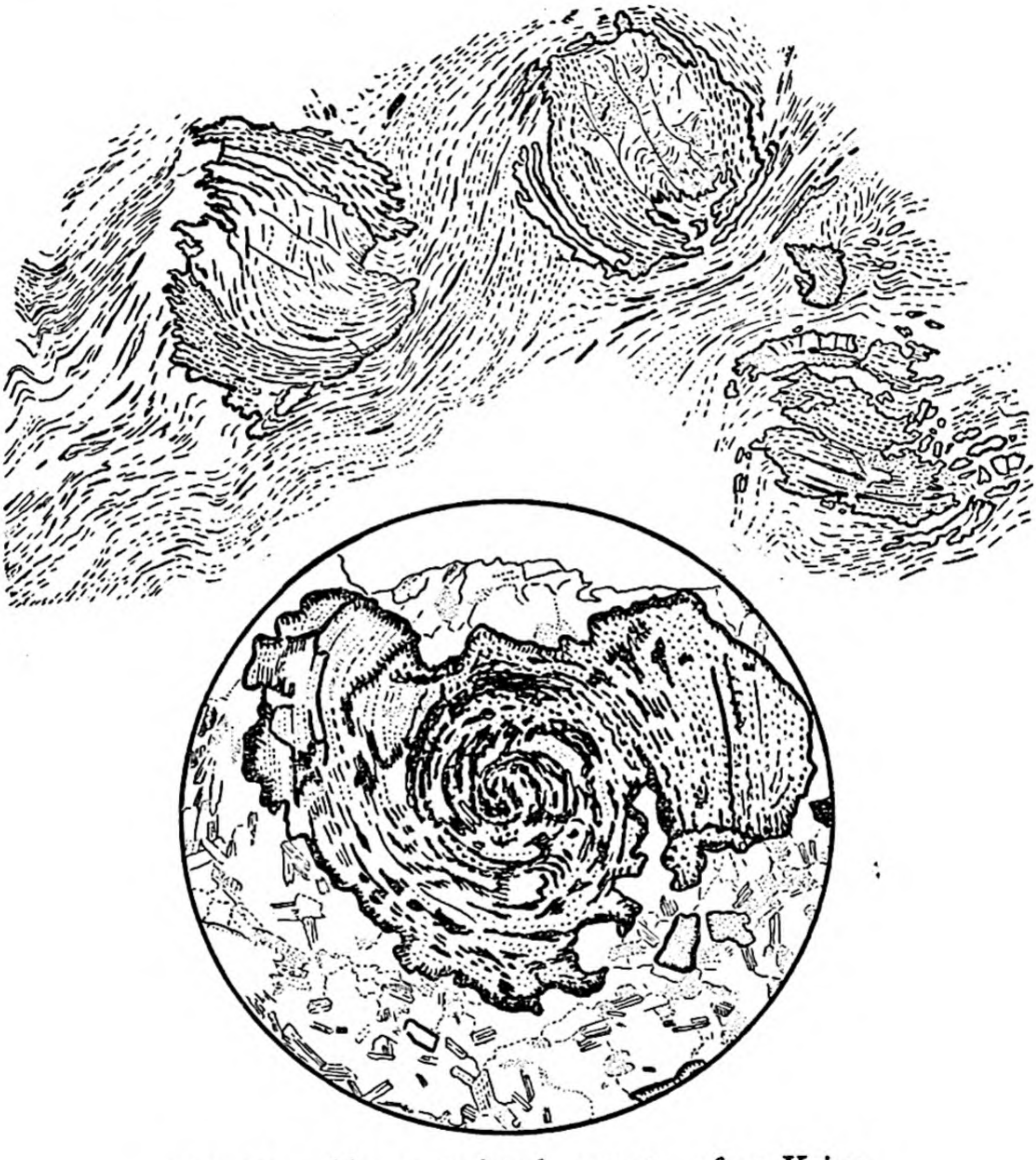


FIG. 10.—Above, twisted garnets, after Krige.
Below, snowball garnet, after Flett.

garnet does—staurolite is taken to be later than garnet (loc. cit., p. 580); in other rocks, biotite is bent and folds can be traced undisturbed through plagioclase and kyanite—the sequence is early biotite, later plagioclase and kyanite (loc. cit., p. 623). Some

of Krige's time-interpretations may not be acceptable to all, but his general demonstration of the complexity of deformation and crystallization in metamorphic rocks cannot be questioned. A beautiful group of his garnets is given in Fig. 10.

In his early and classic investigation of the Taimyr rocks, Backlund (1918) demonstrated an analogous complexity. He describes helicitic folds as becoming petrified by the growth of porphyroblasts; in garnet-staurolite-schists exhibiting helicitic texture, he considers the staurolite to have been formed at the apexes of the old folds, garnet in the compressed limbs. The subsequent history of such a rock is worth following: after this primary helicitic folding and the formation of porphyroblasts, shearing movements produced a banding of staurolite-rich and biotite-rich stripes, these stripes being later folded and thus giving a secondary helicitic texture; still later shearing produced planes along which sillimanite was developed, and continuation of movement led to the reduction in size of the porphyroblasts which, helped by reaction between porphyroblasts and groundmass, eventually resulted in a uniform and even-grained rock. Textural studies of the customary kind are thus capable of yielding an abundance of historical information.

Schmidt (1918) applied exact methods to the study of movements of porphyroblasts with what I consider rather startling results. He is concerned with the *S*-shaped trends in para-crystalline garnet and albite porphyroblasts. The form of the trend in a porphyroblast is a function of the growth velocity and the angular velocity of the crystal; rather sudden changes in the form indicate changes in one or both of these variables, but the *S*-shapes show that in general deformation was of the same duration as growth. Schmidt studies the rotation of three porphyroblasts with the following results:

i. Garnet in mica-schist of the Pfitscher Joch; rotation of 173°, indicating a differential movement of 3·04 units of planes 1 unit apart.

ii. Garnet in mica-schist of Rosskar, Zemmgrund; sharp inflexions in the trend indicate stages in the growth of the porphyroblast, rotation in the core is 183°, in the limbs is 116°. The following garnet history is deduced: growth of

garnet without deformation, rotation of 67°, growth of garnet without deformation, and finally a rotation of 116°. The differential movement indicated is 3·2 units.

iii. Albite in sericite-albite-schist, Moharscharte; the trends indicate first the formation of albite during a steady rotation of 90°, followed by a jerky rotation of 89°, concluded by further growth of albite and a slight backward rotation. The differential movement amounts to 3·13 units.

Becke (1924), in a discussion of Schmidt's results, deals with a rotated porphyroblast that seems to him a better example than any given by Schmidt. Becke, from measurements of diameter and rotation of garnet in an Airolo garnet-phyllite, calculates that for a layer of similar rock 100 m. thick there should be a differential movement of roof over floor of some 560 m. This is the kind of figure that for a brief interval startles, but a few simple reflections restore calm. As Becke himself pointed out, these values are fictitious since they are based upon viewing the rotating porphyroblasts as solid ball-bearings running between parallel guides. A growing snowball-garnet is gradually increasing in diameter and its physical properties are different from those of the groundmass. The images of a growing snowball and of an isotropic garnet finding no rest and condemned to rotate till its history is finished are attractive but may be dangerous if integrated for a layer of any great thickness or extent. It seems to me that the experience of the structural geologist will be of value in the interpretation of trends and rotations especially if, as I suspect, rotation is found to be more characteristic of the porphyroblasts of folded schistose rocks than of rocks of plane attitude. However this may be, there clearly opens up for the research worker a multitude of entertaining and fruitful investigations on trends and the like so soon as the attitude of the porphyroblast becomes firmly tied to the rocks on the ground.

In the time-relations of crystallization and deformation there are three theoretical cases possible: deformation can be pre-crystalline, para-crystalline or post-crystalline (Sander, 1911, 1912, 1930: see Table C). I have used these terms incidentally in the foregoing and I propose now to discuss the relevant criteria and comment on the general validity of the conclusions drawn from them. The

three terms denote time of deformation with reference to crystallization; if we need to indicate the time of crystallization as against deformation we can substitute the three terms post-tectonic, para-tectonic and pre-tectonic, but, as I have never been able satisfactorily to define that rather pretentious word tectonic, I prefer to date deformation against crystallization. When we use these or any other set of terms, we arbitrarily assume that there are strict limits to the times denoted—in many cases, of course, there is none and this aspect of the matter is contemplated immediately.

TABLE C

PRE-, PARA-, POST-CRYSTALLINE DEFORMATION

Pre-Crystalline Deformation: true helicitic textures: old folds pseudomorphed: old *S*-planes control mimetic crystallization: growth-fabrics: preservation of earlier textures.

Para-Crystalline Deformation: snowball-garnets: *S*-trends in albite: micas distorted on outside of arches, not distorted on inside: association of pre-, para-, and post-crystalline minerals.

Post-Crystalline Deformation: mechanical distortion and cataclasis of minerals: distortion of early textures.

We make a start with *pre-crystalline deformation*. Its criteria appear to be decisive; true helicitic textures occur, old folds become petrified, mica laths, for example, outlining and preserving an old fold are undistorted everywhere in arch or trough. The old *S*-planes of bedding, cleavage or schistosity control the subsequent mimetic crystallization and a growth-fabric results. Old textures, perhaps of sedimentation origin, may be preserved. If there were present a preferred orientation in the old fabric it may be more or less completely destroyed—Sahama (1935, 1936) has proposed such an *Entregelung*, or unorienting, for certain quartzites of Greenland and Finland in which patches of oriented quartzes remain relict amongst patches that have lost their orientation as a consequence of post-deformational recrystallization. An old fabric may, on occasion, influence mimetic crystallization by providing suitably placed seed-crystals for the new growth-fabric. But, in spite of all these criteria, certain time-limits concerning deformation and crystallization may still be in doubt. Crystallization may be an entirely later and separate act or it may conclude

a series of related deformations and crystallizations—it may simply outlast deformation. Niggli (Grubenmann and Niggli, 1924, pp. 234–42) and Becke (1924, pp. 188, 193) have expressed the opinion that there is no sharp separation between crystallization and deformation in metamorphic rocks as a whole—metamorphism is an affair, not of successive phases but of a unified process with varying emphasis on the two essential factors. Becke (1924, p.193), again, has questioned the validity of the use of undistorted polygonal arcs of mica as evidence of pre-crystalline deformation; such forms can be produced under directed pressure.

The demonstration of mimetic crystallization thus appears to me to be an operation in which the detailed petrofabric evidence has to be viewed against a background of geological evidence. The preservation of earlier textures, not necessarily of metamorphic origin, the regional attitude of the rocks, the relation of bedding and of schistosity and other field data become significant.

Turning to *para-crystalline deformation*, we find many excellent criteria which we have already sampled in snowball-garnets and albite trends. Snowball-garnets appear inexplicable except as para-crystalline, but albite trends require the most careful scrutiny. Schmidt (1918, p. 301) early emphasized that the tails of the *S*-shaped inclusions should merge without break into the groundmass trend; unless this is observed, growth during movement is not completely demonstrated. Evidence that is to me of a less satisfactory kind is provided by the association of minerals, some showing pre-crystalline, some post-crystalline, deformation and by a single mineral such as mica giving undistorted blades on the inner side of an arch and distorted on the outer side.

But, of course, the very existence of the multitudes of metamorphic tectonites denotes a combination of deformation and crystallization. Complexity arises, however, from two main causes. First, the aggregate undergoing transformation consists of a variety of minerals with different physical and chemical characters, so that a heterogeneity in adjustment is to be expected. To deduce a metamorphic history from one mineral alone is clearly unwise. Secondly, whilst crystallization is, speaking broadly, synchronous with deformation, the production of the rock may be more truly represented as an alternation of the two processes, so that pre-crystalline, para-crystalline and post-crystalline deformations all

become evident. The difficulty arises once again of deciding how great is the time-separation between the episodes. An example of complex para-tectonic crystallization diversified by alternating pulses of mechanical and chemical activity has already been given from the investigations of Turner and Hutton into the albite-gneisses of New Zealand. We shall meet many more, complicated still further by migmatitic and granitizing operations, in later pages.

Lastly, we come to *post-crystalline deformation*. Here the speed of the mechanical processes of cataclasis has exceeded that of new crystallization, so that distortion of early textures, and granulation, undulose extinction, secondary twinning and gliding of the mineral components become evident. Post-crystalline deformation is obviously often not an operation of plutonism at all, and may be a fortuitous episode in the history of any plutonic rock—thus, the Caledonian mylonitization of the Lewisian is clearly not part of the plutonic history of that formation. Its study, however, is often necessary to the plutonic geologist, especially in discussions on the age of the metamorphism, or of the rocks themselves, in many plutonic terrains. It is involved in the debate on the age of the Moine Series of Scotland, as we see later, but now I recall only an example from Greenland; here, Wiseman (1932) held that the metamorphism of the sedimentary rocks resulted from heat engendered by thrust movements, whereas Wegmann (1935*b*, p. 23) pointed out that mylonitization along the thrusts was later than the crystallization of the rocks thrust.

As I see it, then, this pre-para-post nomenclature must be used with a certain care. Before we finally label a rock with any one of the three so-definite labels, we must remember the qualifications and limitations, the complexities and uncertainties, that textural studies reveal—some of these have been set out in the foregoing paragraphs. It may be better to keep the rock without a label than to put a misleading though simple one upon it.

IV. INVERSION

So far we have considered time-differences between crystallization and deformation in the production of the crystalline schists

as indicated by the microscopic relationships of single crystals or elements of texture. We now pass to the examination of time-differences postulated from relationships of quite another order. We have to deal with one of the most interesting aspects of plutonic geology—that concerned with the times of plutonism in general and large-scale movement in general—of plutonism and orogeny. This topic is so vast that, as a beginning, I limit our contemplation of it to the large-scale structures of the more strictly metamorphic rocks. We have to deal with the implications of folding of a metamorphic succession and, especially, with the validity of inversion in the crystalline schists. Certain preliminary inquiries are necessary.

First, we perforce admit the existence of dozens of unequivocal cases of orogeny later than metamorphism. But even in such cases our certainty is often based, not upon plutonic evidence but upon that supplied, almost fortuitously, by stratigraphy. Thus, it is not unfair to suggest that the majority of Scottish geologists are certain that the Lewisian crystallization is broken down by the Caledonian movements in the North-West Highlands, not because of observations on this breaking down but because the Lewisian crystallization is known to be of pre-Torridonian age; that this suggestion is not unfair is indicated by the uncertainty as to what the Caledonian movements have done to the Moine crystallization. We postpone a debate on this local topic and freely admit that orogeny can be later, often very much later, than metamorphism.

Second, if the terms folded and inverted are applied to a series of metamorphic rocks, then two statements are requisite: the one as to what has been folded, whether bedding, schistosity, foliation or cleavage, and the other, as to what was the original attitude of the series that has subsequently become folded or inverted. If we are told that a succession is upside down, we are surely within our rights in assuming that it was once right-way up. Inversion in a non-metamorphic succession of sedimentary rocks implies an act of overturning; it must imply exactly the same in a metamorphic succession. We have, therefore, to inquire into the validity of any metamorphic stratigraphy. Do metamorphic zones obey William Smith's First Law? This is a question that I have discussed on many earlier occasions; I reopen the discussion

today because of the recent French work coming out of the Massif Central.

During my last remarks of last year (Read, 1948*b*, pp. 160, 161, 199, 200, Tables B, E, Fig. 7; this volume, pp. 200-2, 210, 257, Fig. 7), I introduced the ectinites of Jung (1928, 1936, 1938) and Roques (1941), that division of the metamorphic rocks in whose production no visible accession of felspathic material was considered to have taken place. The ectinites occur in superposed zones of isometamorphism, their 'stratigraphical' order being, from below upwards, *gneiss inférieurs*, *gneiss supérieurs*, *micaschistes inférieurs*, *micaschistes supérieurs*. They are not related to orogeny but to geosynclinal deposition and depression in the course of which the original sediments were subjected to increased temperature and pressure and to stretching. If it is considered that the four terms of the series just quoted were originally superposed in a concordant and continuous succession (Jung and Roques, 1938, p. 143), then an observed superposition in a reverse order must indicate a tectonic inversion later than the general metamorphism (loc. cit., pp. 143–5).

The clearest expression of this thesis is given by Roques (1941) in his great memoir on the crystalline schists of the south-western part of the massif. Roques finds it difficult to see in the zones of the ectinites anything but a depth-effect (loc. cit., p. 368). The schistosity coincides with the original sedimentary bedding and was developed when *the sediments were horizontal* (loc. cit., p. 510). This is the normal character of the crystalline schists and is the consequence of a horizontal stretching due, possibly, to frictional drag by convection currents (Read, 1948*b*, pp. 190–1; this volume, p. 261). When the schistosity is no longer horizontal, Roques affirms, then the crystalline schists have been folded at a time later than the impression of the metamorphism upon them (e.g. 1941, pp. 114, 148, 209, 339–69, 370). In fact, we are only able to see the crystalline schists when they have been subsequently folded by compressive orogenic forces (loc. cit., p. 370). In regions of simple structure, as, for example, in the Montagne Noire and the Tulle anticline, the original order of the ectinite zones is still preserved, though in the Montagne Noire the upper members are already strongly folded and have occasionally come unstuck from their substratum (loc. cit., p. 368). In regions of

greater tectonic complication, where the structure is often difficult to decipher (loc. cit., p. 368), the ectinite series is reversed, as in the Rouergue and elsewhere; here the schistosity is no longer plane but has been secondarily crinkled (*froissée*, loc. cit., p. 351) and there is often produced a secondary schistosity oblique to the primary structure, as in the mica-schists of the Lot (loc. cit., p. 340); passages to mylonites are frequently seen, as in the schists and gneisses of the Neuvic massif (loc. cit., p. 369). Roques concludes that the folding and inversion were clearly later than the metamorphism and, further, from the exact parallelism between schistosity and stratification, that the metamorphism affected unfolded beds (loc. cit., p. 370). These propositions of Roques are of fundamental importance in the interpretation of the plutonic rocks.

In the explanation of his recent and most welcome map of the Massif Central, Demay (1946*a*) describes the inversion of the crystalline schists over a vast area between the Rhone and the Argentat fault. In the nappes of the Gier and the Margeride, there appears a *downward* sequence of migmatitic gneisses, mica-gneisses and mica-schists that ends with sericitic slates that are hardly metamorphic at all—gneisses of the highest grade are inverted on low-grade slates. Demay (1946*b*), in his summary of his many researches in the ante-Stephanian arc of the Massif Central, states that the inversion of the metamorphic succession is visible with great clarity all along the arc. This inversion is confirmed by the work of Roques (1941, pp. 351–2), and Demay's (1946*c*) conclusions for the Margeride agree with those of Lapadu-Hargues (1948) that we examine immediately. The sequence of events in the production of the crystalline schists of the Massif Central is, according to Demay (1942), a complex one involving several phases of crystallization, deformation and ascent of magma or migma—this sequence we discuss later when we introduce magmatic or migmatic happenings into the plutonic story. It will be sufficient now to make the general statement that Demay demonstrates a considerable amount of folding and movement with the production of mylonites and their deeper-seated analogues in the crystalline schists.

The clearest statement on inversion in the Massif Central is that of Lapadu-Hargues (1948) for the Margeride. This author admits

(loc. cit., p. 39) that because he is dealing with a series made up entirely of crystalline schists it is necessary, if the tectonics of the region are to be studied at all, to propose a theoretical normal succession and to establish the superposition of its members in relation to magmatic influences ('superposition au sens magmatique du terme'). To erect such a series, it is necessary to adopt what is admitted to be a hypothesis that metamorphism increases with depth. The logic of this proceeding is of interest—to me, at least—and is as follows: every metamorphic series must have at its margin (*mur*) either a less metamorphic or a more metamorphic series. If the series below were to be less metamorphic, it would be necessary to invoke a lateral action of the factors of general metamorphism, which is contrary to all that one has a right to expect ('ce qui est contraire à tout ce que l'on est en droit de supposer actuellement' [loc. cit., pp. 39–40]). Therefore, the series below is more metamorphic. For my part, the logic escapes me.

The succession being established—it is of course the classic phyllite-schist-gneiss sequence—Lapadu-Hargues (loc. cit., pp. 50–3, 54, 60, 62, 73) demonstrates large-scale inversions explained by the existence of a great recumbent fold of the first magnitude (pp. 74, 141). He distinguishes an allochthonous portion of the Massif Central thrust over an autochthonous portion. Having said this, he (loc. cit., p. 141) remembers the difficulties of Alpine tectonics wherein, moreover, a fossiliferous sedimentary series is concerned, and he decides to be cautious in his interpretation of the Massif Central, 'pays cristallins où la succession théorique des séries se base uniquement sur le faciès pétrographique'. But he receives some support from his correlation of magmatic/migmatic depth and metamorphic grade—a correlation I have displayed (Read, 1948*b*, pp. 178–80; this volume, pp. 229–31; Lapadu-Hargues, 1945). None the less, Lapadu-Hargues admits that the whole interpretation of the Massif Central tectonics rests upon a hypothesis which, though doubtless plausible and logical, is still only a hypothesis. He repeats once again the opinion of Boule (1900, p. 10) that *either* the whole ground is inverted in a gigantic recumbent fold *or* the petrographic characters of the Archean are not always of great chronological importance. Finally, but with a little hesitation, Lapadu-Hargues decides for inversion: 'il est

hautement probable, mais non rigoureusement sur, que la série soit effectivement renversée' (loc. cit., p. 142).

When we review this French work, a number of aspects develop for our contemplation. There is the proposition that the original attitude of the crystalline schists as formed in the geosyncline is horizontal and that this attitude becomes subsequently disturbed, even to inversion, by orogenic movements. If there is a time-separation of this kind then it is reasonable, as I have done before, to ask for structural and textural evidence in the inverted rocks. In some measure this is supplied by Roques in records of irregular structures, secondary wrinkling and crumpling, an oblique new schistosity and an incipient mylonitization. How far such records are to be interpreted as a deformation entirely post-crystalline seems to me a matter of uncertainty—a condition not resolved by any evidence provided by Demay or Lapadu-Hargues. The former, as we see later, proposes a succession of deformations and crystallizations that, to me, do not appear to divide themselves into two sufficiently clearly separable classes of events. Lapadu-Hargues, as I have just indicated, at the last decides on inversion but uses a logic that does not convince. Let us examine it.

Lapadu-Hargues's decision depends upon an assumption that a lateral change in the factors of metamorphism is impossible. But his whole succession is controlled in essence by migma–magma influence and whilst, of course, this influence usually increases with depth, it need not always do so. It is not logical to rule out a 'sill-like' invasion by the migma–magma, especially if such an invasion were associated with large-scale crustal movements. In many discussions and in an address elsewhere (Read, 1940; this volume, p. 31), I have repeatedly stated my opinion that metamorphism can increase in grade downwards, upwards or sideways; this opinion still appears to me to be rather more logical than Lapadu-Hargues's. I recall the field evidence for my view. On Ben Klibreck in the great Sutherland migmatite complex, the sillimanite zone overlies the garnet zone—this 'inversion' is related to the abundance of migma–magma material at the upper levels of the mountain (Read, 1931, p. 94). Similarly, Vogt (1927, p. 483) explained the apparent inversion of the so-called depth-zones in the Sulitelma mountains as connected with the greater abundance of granitic intrusions at the higher levels. Both Vogt and I are establishing a

succession 'au sens magmatique du terme', as advocated by Lapadu-Hargues, but, unlike him, we do not admit an immutable 'superposition'.

In this debate we see once again how beset with difficulties the presentation of the history of a metamorphic rock becomes when we force ourselves to use the hard-and-fast time-distinctions implied in any pre-para-post nomenclature. In certain cases, the timing of events is unquestionable—a later orogeny can affect the metamorphic rocks just as it can affect the non-metamorphic sediments, but, in such cases, the orogeny is usually unrelated to a plutonic history; or a mimetic crystallization, dated on geological evidence, can affect a dated folded series. But there remains undecided the chronology of a vast portion of the plutonic rocks. It is here, if anywhere, that the help of petrofabrics is requisite.

It is likely that during the foregoing excursions into many foreign fields this gathering of British geologists has awaited, possibly with impatience, a profound judgement on cognate problems in the plutonic history of the Scottish Highlands. A judgement, whether profound or otherwise, is, however, hardly to be expected in view of the existing lack of information on the sequence of events in Highland metamorphism. We have available no more than a number of broad generalizations poised precariously upon a few pinpoints of fact; statements on Highland metamorphism are largely acts of pure faith. Whoever is proved to be right, those of us proved to be wrong will form a numerous company capable of mutual comfort, for opinions are exceedingly diverse. Nevertheless, it will not be without interest to present a synoptic view of these diversities. I do so now, but make the quite arbitrary decision to exclude for the most part any consideration of the essential migma–magma time-factors of plutonism; these come into the discussion in a later page.

First, there is inversion to be considered. Harker (1932, p. 209) in his treatment of the metamorphic zones of the Dalradian, held that 'a zone of higher-grade metamorphism normally underlies one of lower grade, but this relative position may possibly become inverted by subsequent disturbance'. This rather cautious statement was made positive by others of the Cambridge school. Wiseman (1934, p. 357) was of the opinion that the Dalradian rocks were in a metamorphosed condition before the great

over-folding took place, and that during this operation a number of retrograde changes were impressed upon the rocks. In the Braes of Balquhidder, Tilley (1925) observed the superposition of the garnet zone on the biotite zone and concluded that large-scale movement was prolonged beyond the metamorphic period. For the South-West Highlands, Elles and Tilley (1930) proposed a recumbent folding of the metamorphic zones and concluded that, since the high-grade metamorphic cores closed towards the south-east, the impulses must have come from the north-west; the meta-morphism was earlier than the recumbent folding. For Bailey (1923), however, the large-scale movements had taken place before the metamorphism was completed. At various times I have ex-pressed the opinion that much of the crystallization of the High-lands is of a post-tectonic, mimetic, character. The circle is completed by Kennedy (1948, p. 231), who deduces an anticline of the metamorphic zones in the south-western and western Highlands, an arrangement 'obviously not related to the surface tectonics of the region but . . . to some major element of basement structure'. All these observers have viewed the Dalradian rocks as monometamorphic and have, indeed, unconsciously assumed that all their mineral constituents were made 'in one casting'. Dalradian plutonic history cannot be so simple as this. It should be recalled that for part at least of the Dalradian terrain a more complex sequence of mineral formation has been proposed: Doris Reynolds (1942, p. 50), in her discussion of the Antrim albite-schists, to which I make extended reference later in these pages, considers that the biotite that gives the schistosity of these rocks was of early production presumably contemporaneous with deformation, but that the albite formation took place after the folding movements had ceased. If this interpretation is correct it is still possible that only one metamorphic episode is involved; the albite-schists are monometamorphic rather than polymeta-morphic.

Foreign opinion on the Dalradian history adds to the diversity of the native. Thus, de Lapparent (1935) regards the Dalradian as clearly polymetamorphic and has proposed two metamorphisms in the Loch Lomond and in the Schichallion districts. Suess (1929, 1931, 1934, 1938*a*) holds that polymetamorphism is revealed in almost every specimen of Dalradian rock (1931, p. 77), the

condition of the rocks resulting from continuous crystallization during movement: that is, from *enorogenic* metamorphism (1938*a*, p. 323). This Dalradian metamorphism is different from that of the Moines, whose state is due to heating without deformation and whose crystallization is essentially mimetic—the Moine metamorphism is *periplutonic* (1938*a*, p. 324). So different is the Dalradian from the Moine metamorphism that Suess argues that there must be a tectonic break between the two series.

This is not the place to pursue the entertaining questions of the date of the Moine Series as sedimentary or as metamorphic rocks, for these are only in part a plutonic matter. I have already put my view in some detail (Read, 1934*c*), and J. Phemister (1948, p. 34) may be consulted as an antidote—all I need do here is to illustrate once again the diversity of opinion on a Highland problem. The main debate concerns what is happening to the Moine rocks at the Moine Thrust. Is the general metamorphism of the Moine being broken down adjacent to the thrust or is what is seen at the thrust a low stage in the building up of the Moine metamorphism? I hold that the first is the case in Sutherland, others hold that the second is the case in Sleat and Glenelg. It is possible, of course, that there is no unique solution and that, as elsewhere, the crystallization and the deformation are not single and simple events. As I have said already, little is known of the metamorphic history of the Highland rocks, whether Lewisian, Moine or Dalradian—a condition leading to easy generalization. A selected small area needs to be studied in detail. As I contemplate the position, I echo the desire from *Moby Dick* for time, strength, cash and patience.

V. THE INTERPRETATION OF METAMORPHIC HISTORY

In earlier sections of these remarks, we were concerned first with the building up of metamorphic history from the records of crystallization and deformation elucidated by petrographic methods, and then with the significance of these records in the interpretation of certain crustal arrangements of a large order. Our concern has necessarily been more with the detail of single or short episodes than with the broad succession of events in the

making of the metamorphic rocks and of vicissitudes that may have overtaken them. I have now to discuss the extension of these interpretations to the whole varied history of a selection of metamorphic terrains some of which are of classic and abiding interest; in doing this, we shift the emphasis from the laboratory and the thin section to the field exposure and the geological map and, accordingly, the difficulties of interpretation become necessarily greater. As I have said, many of the incidents recorded in the metamorphic rocks are not plutonic at all, but such incidents may be of great value in dividing the stretches of time more relevant to our discussion. It is still convenient to exclude, so far as is possible, the migma–magma factor in these histories.

I begin by repeating samples of the metamorphic history of

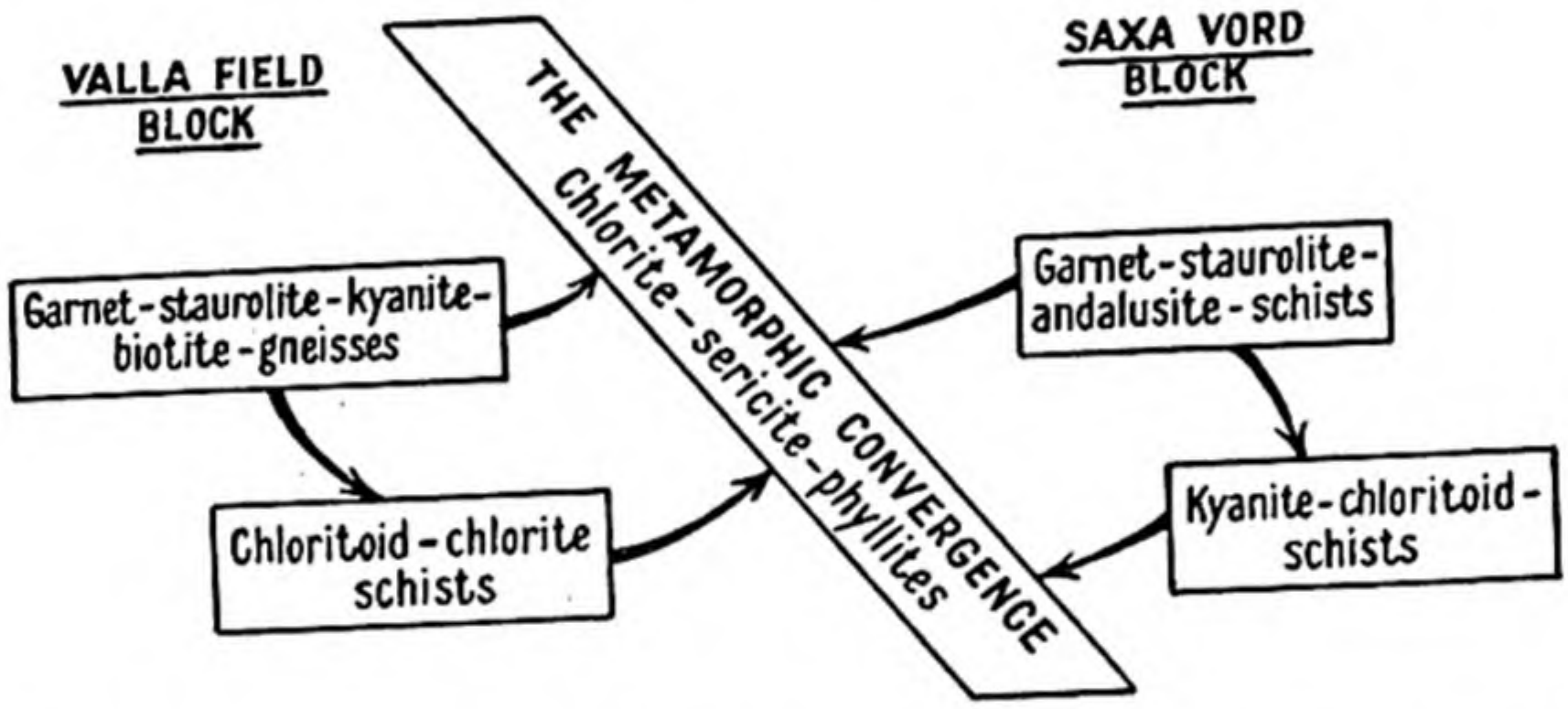


FIG. 11.—Unst metamorphic history and metamorphic convergence.

Unst in the Shetlands, as these are relatively simple, for the most part indisputable and drawn from my own geological repertoire (Read, 1934*a*, 1937*b*). This excellent island is formed of a number of structural units, made of a great variety of metamorphic rocks of igneous and sedimentary parentage, and separated from one another by patent dislocations. To illustrate the interpretation, I can best confine our attention to two structural units and, in these, to the evidence afforded by a single rock-type, that of pelitic composition. In the one structural unit, that of Valla Field, a first metamorphic episode resulted in the production of garnet-staurolite-kyanite-biotite-gneisses and was followed by a second during which this assemblage was not stable and a chloritoid-chlorite-muscovite-schist was made from the gneiss. In the second structural unit, that of Saxa Vord, a first metamorphism gave

staurolite, andalusite and garnet, and on this was superposed a second characterized by the formation of chloritoid and kyanite. Along the mechanical junction of the two structural units, all pelitic rocks adjacent to the belt of special movement are converted into chlorite-sericite-phyllites—a type example of *metamorphic convergence*. No matter how different the previous histories of the rocks may be, in this third event affecting both structural units they converge to a common type. The sequence and diversity of the Unst metamorphic episodes is outlined in Fig. 11.

In Unst the third stage in the history, which resulted in the production of the convergent types of epizonal character, was impressed in a narrow zone readily apparent because of structural discontinuities such as thrusts and imbricate planes and marked as a whole by a topographic depression. Similar obvious dislocation belts with a characteristic modification not easily attached to any plutonic event are, of course, abundant. We can instance the convergence along the Moine Thrust in Sutherland or the production of the Pfahlschiefer, sericite-chlorite-epidote-phyllites, along those great straight fractures, such as the 160-km. long Bavarian Pfahl, that break up the Hercynian arcs of central Europe. Long ago, Becke (1892) mapped and described certain chlorite-gneisses developed along the Ramsau Line in the Altvatergebirge of Silesia and interpreted them as a movement horizon and not a stratigraphical one; on this horizon many different metamorphic rock types had become converted into the convergent chlorite-gneiss. Again, as von Bubnoff (1921) and others have shown, the gneisses of the Rhine horsts are traversed by belts of schistose rocks with a schistosity oblique to the old gneissose structures —such belts can be interpreted as zones of special movement leading often to overthrusting. Conversely, juxtaposition of blocks with different metamorphic histories can be readily accepted as indicating a mechanical contact between them; thus, along the Otzberg dislocation in the Odenwald, the Bollstein gneiss dome is brought against folded metamorphic rocks of an entirely different facies (e.g. von Bubnoff, 1922). I have given these examples to emphasize the importance of metamorphic history, facies and convergence in geological interpretation.

Along many of the belts of what I term dislocation-metamorphism so far mentioned, there are produced rocks that are

often of lower metamorphic grade than those from which they are derived; in Unst, for example, mesozonal gneisses and schists of the earlier episodes are converted on the convergence into epizonal phyllites. Accordingly, it is not surprising that retrogression or retrometamorphism has been applied to such episodes. This is a useful term, but one that appears to me to be not without its defects. It belongs to the class, which includes degradation and others, that seems to give a sort of social scale to metamorphism and, besides, its use presupposes progression and this, as we have already seen, is a matter for debate. Because what is implied by retrogression as customarily used is a common event in the history of plutonic rocks, it is necessary, for the proper description of such histories, to consider what is contained in the two facets of retrometamorphism designated by that lovely pair, diaphthoresis and phyllonitization. It may help future work if we attempt to delimit these processes as closely as possible.

Diaphthoresis was introduced by Becke (1909) to denote what had happened to the Kellerjoch gneisses in the Hohe Tauern; these rocks are epizonal assemblages of quartz, albite, chlorite and sericite but they retain mineral and textural relics of katazonal character. High-grade rocks had been destroyed and for this process a convenient term was diaphthoresis, from the Greek to destroy. Becke, the originator of depth-zones, naturally interpreted diaphthoresis as resulting from the orogenic displacement of rocks of a deep zone into a shallower one, with consequent adjustment to the new physical controls. Perhaps, after all, it was Becke who originated a social scale for the metamorphic rocks, since he talked of diaphthorites as having seen better days ('die vielleicht früher einmal bessere Zeiten gesehen haben'). Beautiful examples of diaphthoresis have been described by Kölbl, Kieslinger and others; we refer to some of these and to others later. As E. B. Knopf (1931, 1935) has emphasized, diaphthorites are tectonites in that they have been deformed by differential movements whereby molecular rearrangements are promoted either by increase in temperature or by the provision of privileged paths or planes; simple block-uplift does not produce diaphthoresis—if it did, no katazonal rocks would be available at the earth's surface. The essential process of diaphthoresis is therefore retrogression of metamorphic grade under deformation. Changes such as the

chloritization of biotite and garnet that can be ascribed to hydrothermal activity associated with pegmatization (e.g. Cloos and Hietanen, 1941, pp. 135, 136) are clearly not to be included in diaphthoresis, though the mineral transformations are essentially the same.

We may now proceed to consider the second term connected with retrogression, namely phyllonitization. The genesis of phyllonites (Sander 1923) is indicated in their name, which is a combination of phyllite and mylonite. Phyllonites are produced by the mylonitization, with varying amounts of new crystallization, of any originally coarser-grained rock, no matter what the origin of this may be. The result is a rock of phyllitic appearance, often with a lenticular structure. Many phyllonites are diaphthorites, but not all; for example, a coarse granite could become a phyllonite but not directly a diaphthorite. If these words are to be used, these distinctions are unfortunately necessary.

We have seen that metamorphic convergence produces a belt of metamorphic rocks common to the marginal portions of the structural entities involved. It is possible to conceive a convergence at a mechanical contact of katazonal metamorphic rocks with rocks scarcely metamorphosed; we may find examples of this in mechanical junctions of Lewisian and Torridonian in the northern Highlands. The convergent part of the katazonal rocks might be diaphthoretic phyllonites, that of the originally non-metamorphic rocks would be non-diaphthoretic phyllonites. This illustrates the correct way to use these terms and leads on in a moment to certain entertaining developments. Meanwhile, it has become evident that the interpretations as diaphthorites or phyllonites of certain groups of rocks may be of fundamental importance in the unravelling of the geological history of the crust.

If we contemplate the possible thermodynamic envelopes in which, unlike Unst and the other cases cited, enhanced temperature and a greater activity of solvents, mineralizers or emanations may be more significant than directed pressure, we can readily expect conjunctions of different metamorphic grades. These may provide remarkable sequences of metamorphic intercalations and superpositions that, without a knowledge of the relevant metamorphic histories, appear haphazard and meaningless. As conditions vary, we may approach the more definitely plutonic

aspects of this present inquiry and, on occasion, find ourselves confronted with 'normal metamorphic' sequences of the textbooks that yet conceal totally different histories in their different elements. Elaborating what has been said in the previous paragraph, we may envisage a katazonal block overriding an epizonal block with constructive metamorphism of mesozonal type in the belt of movement and thus producing an upward succession of epizone, mesozone and katazone; we may even contemplate the production in a region like the North-West Highlands of an upward succession from non-metamorphic rocks to those of the highest grade. Such false successions will obviously provide unlimited opportunities for wrong conclusions on the structural and metamorphic histories and their relationships in that crustal segment. To illustrate these aspects of interpretation, I select for consideration the arrangements of the rock types along the Moldanubicum-Moravicum boundary in the Hercynian massifs of Austria and Bohemia as this is viewed by F. E. Suess and his school.

The region we are concerned with includes the Austrian Waldviertel made classic by the researches of Becke, and stretches from Krems on the Danube northwards astride the border of the old provinces of Moravia and Bohemia. We are more particularly interested in the southern part of this area between the Danube and the Thaya, and a sketch-map of this part is given in Fig. 12. The relationships between the rock-groups are here presented in the merest summary; for the most part I follow the syntheses of Suess (1926, 1937, 1938*b* and literature therein cited), but I must emphasize that certain of Suess's simplifications and interpretations have been expanded or questioned. The broad outline of his work is, I believe, accepted by competent critics.

In this region, two great structural units are involved, on the west the Moldanubicum, on the east the Moravicum, separated by a movement belt along which the Moldanubicum has been thrust eastwards over the Moravicum. More than half of the Moldanubicum is made up of granitic rocks around the batholiths of which the country-rocks smear themselves concordantly without dominant tectonic strikes. It is a terrain typical of Suess's zone of intrusion tectonics. The country-rocks are possibly metamorphosed derivatives of the Bohemian Barrandian sediments of 'Algonkian' and Lower Palaeozoic age and of their associated

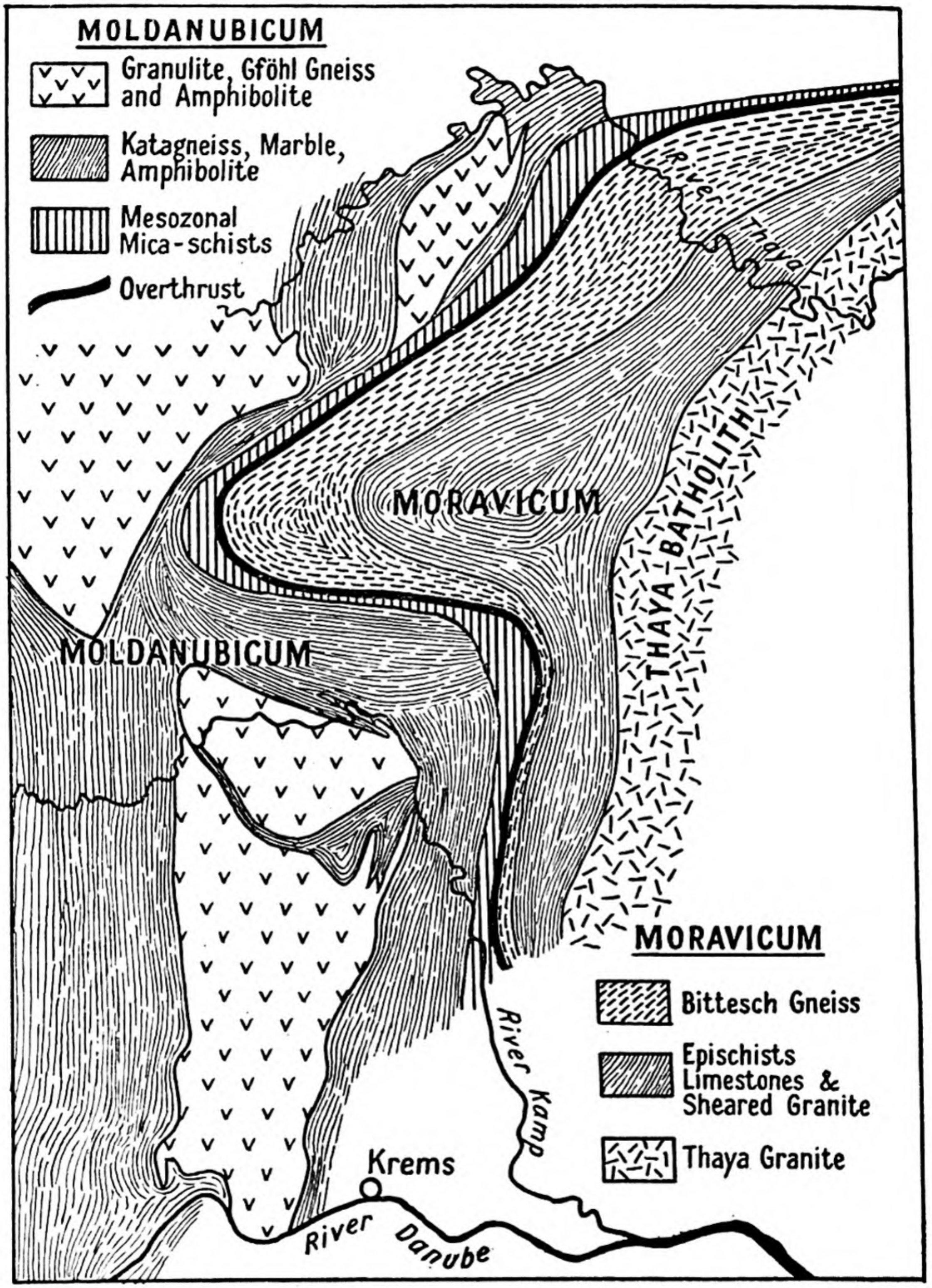


FIG. 12.—The Thaya window. (After F. E. Suess and others.)

igneous rocks. Their metamorphism is katazonal of eclogite facies, and they include types such as augen-gneisses, eclogites and granulites, showing as their final impress a mimetic post-deformation crystallization. It is probable that their metamorphic history is much more complex than presented by Suess, as shown for certain members by Waldmann (1927), for example; the time-relations between granite emplacement and metamorphism are especially topics for discussion (e.g. von Bubnoff, 1930, p. 462). But, for our present purpose, it is sufficient to accept the broad picture of the Moldanubicum as a katazonal metamorphite-migmatite-granite block. The boundary between this block and the Moravicum underlying it to the east is apparently sharp and distinct. As the Moldanubicum is traversed towards this boundary, its katagneisses give place to a broad zone of mesozonal rocks, coarsely crystalline mica-schists with porphyroblasts of muscovite, garnet, staurolite and kyanite and with a structure strictly parallel to the Moldanubicum-Moravicum boundary. Kölbl has given details of this zone as it is developed at the Messen nose of the Thaya Moravicum. Rocks of suitable composition in the zone develop large plates of muscovite, and this mica is abundant throughout the zone; this is the *Deformationsverglimmerung* of Sander or, as Suess puts it, the rocks have contracted *muscovititis*. This mesozonal mica-schist belt is interpreted as a deep-seated diaphthorite developed at the base of the Moldanubicum as it moved eastwards over the Moravicum. It may at once be remarked that there is abundant criticism of this view. Becke objected that the supposed parent rocks of the mica-schists were very different chemically from these—an objection met by Suess by reference to the geological evidence, seen in the field, of the relations of the mica-schists to the visible structural contact between Moldanubicum and Moravicum. It may be advanced, as Preclik has done, that the belt of movement provided privileged paths for transfer of material, possibly from aplitic sources. Other objections dealing with the superposition of katazone on mesozone, raised by Hinterlechner and Limbrock, for example, are in my view of little weight (see, for a discussion of this matter, E.B. Knopf, 1931, pp. 23–6).

The mesozonal mica-schists are followed structurally below by the Moravicum, which consists mainly of the metamorphic derivatives of the Sudetic Devonian now in an epizonal or, more

rarely, mesozonal condition; granites form the cores of the structural domes of Thaya and Schwarzawa. Below the schist mesozone of the Moldanubicum base comes the widely developed Bittesch gneiss, a sericitic augen-gneiss with structures parallel to those of the Moldanubian mica-schists and therefore to the contact between the two structural units. The gneiss shows a muscovititis similar to that of the mica-schist zone; it should be noted that the undeformed nature of the large mica plates has suggested to Königsberger that these are not of deformation origin but telepneumatolytic—but Suess finds no relation between their distribution and that of any granitic bodies. The Bittesch gneiss is a blastomylonite with a para- or post-crystalline deformation. Inwards from the gneiss margin of the Thaya dome there follow belts of flaser-granite and of phyllites and schists, usually epizonal but in places carrying garnet and staurolite, and then the augen-gneiss rim of the massive Thaya granite core. Waldmann (1928) in his study of the Thaya dome concludes that the Moldanubicum is clearly separable from the Moravicum by internal structure, components and metamorphic history.

Suess, (1926, p. 202) gives the following summary of his views on these matters: the Moldanubian katazonal block moved slowly over the Moravian domes, a broad zone at its base being thereby converted into the crystalline facies of the mesozone; by the same deformation the Moravicum just below the thrust zone was converted at this depth into the same metamorphic facies 'dadurch hat an der Grenze eine Annäherung der metamorphen Fazies beider Gebiete stattgefunden'. The result is the paradox that in the Moravicum the deepest structural zone shows the least depth-metamorphism (von Bubnoff, 1930, p. 500). We may represent the conditions diagrammatically in Fig. 13 in a manner similar to the case of Unst. Along the convergence, diaphthoretic phyllonites are produced in the Moldanubicum, phyllonites in the Moravicum.

Further examples of the use of metamorphic history in geological interpretation or, at least, in the discussion of geological interpretation, could be multiplied without end. Numberless examples arise in the investigations of the Hercynian horsts of central Europe: of these, I can mention only the studies of Berg (1912, 1923) in the Riesengebirge, where the history begins with crystallization in a

granite-contact, moves on to mesozonal schist-making and ends with diaphthoresis of epizonal type. The great problem of the geological relations of the Münchberg gneiss may be solved from the metamorphic histories of its constituent parts: here a katazonal gneiss body has an epizonal margin against non-metamorphic Palaeozoic. Two major alternatives have been proposed, the one suggesting a klippe of katazonal rocks with a diaphthoretic epizonal

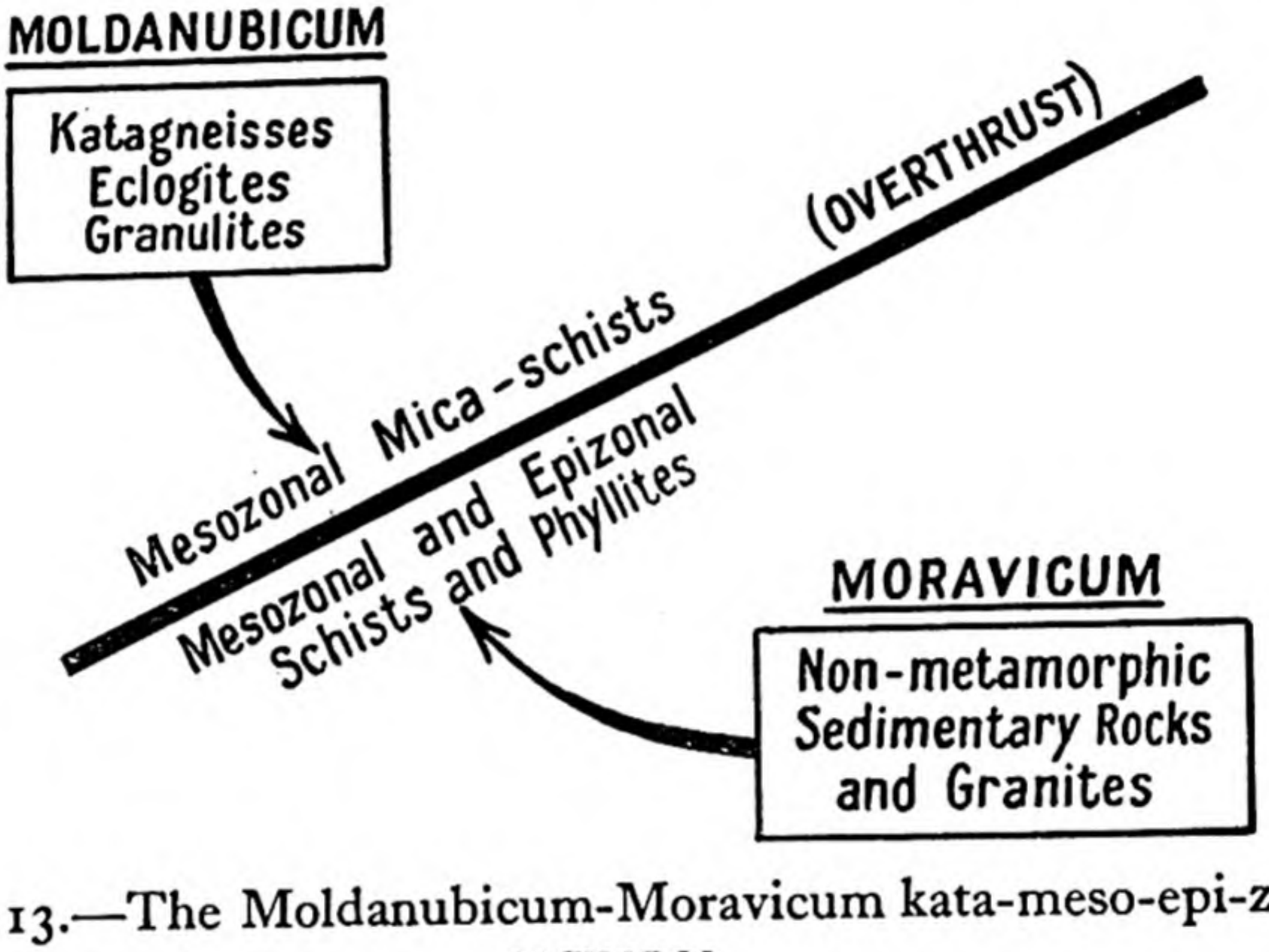


FIG. 13.—The Moldanubicum-Moravicum kata-meso-epi-zonal sequence.

base above the thrust-plane, the other an autochthonous intrusive mushroom. In all these problems it is not place that is more important, but the timing of events. As my final example, I may refer once more to Cloos and Hietanen's (1941) investigation of the 'Martic Overthrust' region of Pennsylvania and Maryland, as here it is metamorphic similarity in the constituent parts that is decisive. The fact that mineral orientation originated after the present position of the sequence had been established shows that any thrusting is pre-metamorphism.

VI. POLYMETAMORPHISM OR MONOMETAMORPHISM

From what has been said in various sections of these remarks, we approach the problem of the subdivision of the history, metamorphic

or otherwise, of a rock. We can separate two ideal cases. In the first, the series of episodes of crystallization and deformation, though alternating and not strictly coeval, can reasonably be considered parts of a unified whole; though we may here be busy with the details of a lengthy and complex sequence of deformation, rotation of porphyroblasts, replacement, crystallization and other operations, we are satisfied that all these operations belong to one self-contained act in the history of the rock—the rock is monometamorphic. Second, there is the case of two or more unified acts that are separable from one another and present no obvious genetic connexion—the rock recording such a history is polymetamorphic.

There are certain obvious cases in the interpretations of which there can be little or no doubt. For example, Cheng's sequence of events in the Sutherland migmatites appears to be a unified act in which many of the minor episodes are consequences of internal adjustment rather than of external controls. Again, the special types of transformation that are styled dislocation or thermal metamorphisms can be superposed on true metamorphism to give rocks that are readily interpretable as polymetamorphic. For example, the breaking down of the Lewisian eclogite facies by the Caledonian dislocation metamorphism in the North-West Highlands, the formation of kyanite-schists from andalusite-hornfels around the Inchbae granite of Ross-shire, and the reverse change in aureoles of the Newer Granites of Aberdeenshire may be interpreted as polymetamorphic operations. Nevertheless, it is yet possible that in some of these simple-looking cases, the separate metamorphisms are more closely related than they appear—a matter that will become clearer when we have considered the Granite Series.

In this inquiry, we should rid ourselves, so far as is possible, of such notions as those of stress and antistress minerals, depth-control of metamorphic facies and the like, otherwise we shall be tempted to accept as polymetamorphic a history that is really monometamorphic. To illustrate this point, we may refer to the sillimanite-gneisses and kyanite-gneisses found in many thermal aureoles, such as those of certain Galloway granites or of the Bushveld lopolith, or to the Ecker gneiss of the Harz—in such cases a polymetamorphic 'stress' metamorphism might be invoked

as superposed on an 'antistress' thermal metamorphism, and invoked wrongly. As described by Erdmannsdörffer (1911), the Ecker gneisses are entirely different from normal hornfelses, being coarsely foliated and schistose rocks with streaks of different mineral compositions—but their geological relationships are definitely those of an aureole and they can be interpreted, not as due to a second 'dynamic' metamorphism following a first 'thermal' metamorphism, but to one act of a kind not usual in thermal aureoles. We may suspend judgement on the case of the aureole of the Ross of Mull granite where sillimanite-gneisses are developed. Bosworth and Bailey conclude that the sillimanite is of one generation, Harker (1932, p. 326) distinguishes two generations, one of regional and a later of thermal type.

As we can work only with the material that the rocks provide, a decision on polymetamorphism or monometamorphism is often impossible to make. One may be able to deduce from fabric studies that entirely different movement-pictures belong to entirely different portions of a metamorphic history, but in many cases one can make no more than suggestions or informed guesses. Thus, for Unst, I suggest that the first two episodes in the Valla Field Block are distinct and widely separated because the structural trends of their products are divergent, but for the Saxa Vord Block, I guess that the first two episodes are very closely connected. Again, how wide a separation is to be made in the metamorphic events affecting the different parts of the Moldanubicum-Moravicum conjunction? If nothing but Devonian rocks are involved in the Moravicum, then we could decide on a monometamorphic history for that block; but the possibility is by no means ruled out that the Moldanubicum includes Archean components, and polymetamorphism is therefore to be expected. It becomes clear that so soon as we leave the small-scale time-differences of the unified acts we must rely on geological evidence based upon observations not of thin sections but of crustal units of a large order.

If we view metamorphism as a branch of plutonism, it is reasonable to regard an approach to surface crustal conditions as an event that may well separate two plutonic episodes. The imposition of dislocation-metamorphism at shallow depths with the production of diaphthorites can be taken as closing a particular

plutonic chapter. Apparently still more final terminations are put by the uplift of plutonic terrains actually to the surface—a happening employed by Sederholm in his division of the Finnish Archean. A third and more valuable time-limit is given by a phase of basic dyke intrusion, representing a high-level tensional effect and drawing on simatic material—and so entirely different from any possible plutonic event. The classic example of the third criterion is provided by the Lewisian of the western seaboard of the Highlands, where, for example, the impression of the eclogite metamorphism well displayed in the Scourie district of Sutherland is separated from that of the later amphibolite metamorphism developed to the north by an episode of basic dyke intrusion. One of these basic dykes is, of course, that which provided the material for the fundamental work of Teall on metamorphism. So different is the geological setting of eclogite and amphibolite production on the one hand and of basic dyke intrusion on the other that we are justified in breaking up the Lewisian metamorphism into at least two separate acts. I trust that eventually we shall outgrow the too compendious name of Lewisian and speak, perhaps, of the Peachian period separated from the Cloughian period by the Teallian incident. In the Waldviertel Moldanubicum, Waldmann (1927) has shown that the plutonic history of the Gföhl gneiss is separable into two portions by an episode of basic intrusion; by analogy with the Lewisian style, I consider that the two portions are genetically distinct—the Gföhl gneiss is polymetamorphic.

As I have said, the proper partition of metamorphic history may be a matter of difficulty. It is by the use of geological evidence of the kind we have noted, reinforced by considerations of the plutonic events associated with the Granite Series that we examine later, that the difficulties may be overcome.

VII. TIME AND MIGMA–MAGMA

So far in this consideration of plutonic history I have for my own convenience kept out of the story what I have incidentally referred to as migma–magma. This might have led to a performance of Hamlet without the Prince of Denmark, for the whole operation of plutonism, with its trinity of granitization, migmatization and

metamorphism, depends upon the activity of emanations, mineralizers, ichors, juices, migmas, magmas and the rest of the nebulous host. We must give to the story its essential core. This, as I have said, I have called the action of migma–magma. Before even the query is put, I agree that this term brings to mind many similar combinations, such as mumbo-jumbo and hocus-pocus, that are of no great standing. Nevertheless, it will be conceded by all that there are migmas and magmas and that migma may become magma—so that migma–magma is not such mumbo-jumbo as it first appears. Igneous operations form a small part of those we are now to consider. We are to deal with the time-relations of crystallization, deformation, granitization, migmatization, metamorphism, intrusion and orogeny—we watch go round all the wheels of plutonism.

The idea that in what I now call plutonic terrains there were granitic rocks of many different ages which revealed their time-relations to orogenic events by their textural and structural characters is one of long standing. From the many available developments of the idea I choose one from our own country. Half a century ago, George Barrow had advanced a complex sequence of relationships between various styles of movement, the uprise and crystallization of magma and the impression of different metamorphisms in the South-East Highlands. His view (Barrow, 1892, 1893) that certain round-grained oligoclase-gneisses were produced from a granitic magma by the straining off of a potash-rich pegmatitic fraction 'during or towards the end of a powerful earth-movement' is one of the earliest proposals of filter-press action controlled by orogeny. Later, in the Blair Athol region, he (Barrow, 1905) separated and interpreted a number of intrusions of diverse relationships: his Early Granite Magma included three phases—first, the Ben Vuroch granite, which was solid before crushing and therefore shows extensive cataclasis; second, the Tilt and Blair Athol bodies, which are protoclastic, the material of final consolidation of the original magma being still liquid during movement and hence squeezed out; and third, pegmatitic material which showed no trace of either cataclastic or protoclastic structure and so must have remained liquid until movement had ceased. He separated, too, an Intermediate Magma, revealed as a multitude of non-foliated

granitic or pegmatitic veins which, from their texture and relationships, he interpreted as 'essentially of the same age as the crystallization of the metamorphic rocks, and . . . in fact, a part of the phenomena associated with their metamorphism' (1905, p. 100). Northwards in Braemar, Barrow (1912*a*) illustrated his three broad classes of igneous rocks established with reference to the regional earth-movements: 1, entirely earlier than the oldest folding were intruded the basic epidiorites; 2, later than the folding but contemporaneous with the great shearing movements which, Barrow held, had affected the rocks, came the Old Magma, a series of granitic rocks practically of the same age as the regional metamorphism; and 3, the Newer Granites were intruded entirely later than all movements. For Cromar, I (Read, 1927) have separated out the same three phases under the names of the Geosynclinal or Greenstone Phase, the Movement Phase and the Post-Movement Phase.

Barrow (1912*a*, p. 24) has summarized the plutonic history of the Glen Muick region: the oldest of the Older Granites is an oligoclase-biotite-gneiss which welled up before the crushing movements and before strain-slip cleavage had been developed in the rocks—it produced the hornfels type of alteration ('used in the structural sense'). This early induration was largely broken down by later crush-movements which resulted in lenticles of the hardened material persisting in micaceous folia. Later intrusions of the Older Granite raised the whole area to a high temperature and 'the crystallization now seen in all the rocks is essentially of the age of this maximum temperature'. Whether Scottish geologists accept all Barrow's interpretations or not, they must acclaim with me this illustration of his pioneer genius; it is in some measure unfortunate that he worked in Scotland when all the available limelight was focused on the North-West Highlands—but he will receive his medals, though tardily.

In the twenties and thirties of this century, proposals similar to Barrow's were synthesized and systematized by a number of workers in different parts of Europe, among whom were Scheumann and Kossmatt in Germany, Wahl, Wegmann and Kranck in Finland, Demay and Raguin in France, Read in Scotland. We can best begin the study of such syntheses by examination of the schemes of Scheumann (1924, 1932; see also von Bubnoff, 1930,

p. 603, and A. Knopf, 1948, p. 657). As a preface to his 1924 study of the Frankenberg gneiss, Scheumann advanced the view that the phases of magmatic development before, during and after a great folding epoch formed a regular sequence closely related to the tectonic events; he named the three phases prototectonic, syntectonic and apotectonic. He proceeded to dissect the events in the Thuringia-Saxony block and separated out two independent magmatic-orogenic cycles, one pre-Variscan, one Variscan. We can represent his results in the Table D below.

TABLE D

MAGMATIC-OROGENIC CYCLES IN THE THURINGIA-SAXONY BLOCK

(Scheumann)

	Pre-Variscan Cycle	*Variscan Cycle*
Prototectonic	Bohemian spilites of the Algonkian. Amphibolite-eclogites.	Ophiolites of the Devonian with keratophyric end-products.
Syntectonic	Orthogneisses of Frankenberg and Münchberg.	Grey orthogneiss, pegmatite-gneiss, mixed gneisses of the Erzgebirge. Granulite-gneisses, older granulite-gneisses, older granulites, granite-gneisses, mixed rocks of the Granulit-Gebirge.
Apotectonic	Strongly deformed porphyrites (sericite-phyllites).	Discordant granites of the Fichtelgebirge, Erzgebirge, etc.
	(The whole series shows soda-dominance.)	(The whole series shows potash-dominance.)

It is necessary to point out immediately that the contents of the Variscan syntectonic phase comprise an immense array of different rocks with what seem to me divergent plutonic histories. Further,

many items in this syntectonic phase may not be Variscan at all; thus von Bubnoff (1930, p. 605) holds that the Variscan age of the grey and red gneisses of the Erzgebirge and of the granulites is by no means proved. He is disturbed by the crowding of events, recorded by Scheumann's proposed syntectonic rocks, into the short Sudetic time-interval of the Variscan orogeny. But he accepts the general principle of magmatic-orogenic cycles, and indeed suggests that a third Caledonian cycle may come in between the two proposed by Scheumann.

We, too, may accept the general principle, at least for discussion, and we can set out Scheumann's three phases as in Table E (Erdmannsdörffer, 1936; A. Knopf, 1948).

TABLE E

MAGMATISM AND OROGENY

(Scheumann, Erdmannsdörffer, A. Knopf)

Prototectonic: extrusion and shallow intrusion of basic magma with keratophyric end-products.

Syntectonic: movement of granitic or granodioritic magma into deformed anticlinal cores to give gneiss domes encircled by migmatitic and metamorphic zones; the volatile components of the magma are dispersed and not localized, and lamprophyres are lacking.

Apotectonic: emplacement of granite with normal contact aureoles, pneumatolytic concentrations, and eventually with extrusive equivalents and a final lamprophyre phase.

As was inevitable, similar propositions were developed for the vast plutonic complexes of Fennoscandia. So long ago as 1916, Geijer had contrasted the cross-cutting Younger Granites of the Swedish Archean with the Older Archean Granites which formed anticlinal batholiths, with gneissic margins, emplaced during folding. For these concordant bodies, the dome structure had resulted from upward push of the magma—'the form of the massifs always shows a very marked dependence on the structure of the surrounding rocks'. Eskola (1932*c*), in his discussion of the origin of granitic magma, advanced the general proposition of the squeezing-out of anatectic magma during orogeny and divided the Finnish granites into four magma-tectonic types: *synkinematic*, such as the older Archean granites like Orijärvi, granodioritic in

cast, concordant in structure and clearly intruded into highly mobile surroundings; *hyperkinematic*, exemplified by the Lapland granulites; *late kinematic*, illustrated by the potash-rich palingenetic Hangö granite; and, lastly, the Rapakivi *post-kinematic group*. Later, Wahl (1936) separated geosynclinal, primorogenic, seroro-genic and postorogenic phases of Finnish magmatic activity. In their studies of the Svecofennids in Finland and the Caledonids in Greenland, Wegmann and Kranck (1931; Wegmann, 1938; Kranck, 1937) have given many examples of granitic intrusion, granitization or mobilization that have operated simultaneously with movement. The synkinematic granites of the Kongsberg–Bamble district of Norway are described by Bugge (1943, p. 114) as varying from coarse porphyritic granites and augen-gneisses to fine- and medium-grained granites that grade into granitic gneisses.

The relation of magmatic phenomena and orogeny has always had the solicitude of the French, and recently this study has been elevated to a new branch of geology—*Microtectonique et Tectonique profonde*—by its foremost French exponent, Demay (see especially Demay, 1942, and bibliography therein). As a result of his vast experience in the Cevennes and other crystalline regions, Demay concludes that the great gneiss terrains attributed to regional metamorphism could be formed only by the alternating or simultaneous play of dynamic and magmatic actions belonging to the same orogenic period. His massive memoir of 1942 has the explanatory sub-title *cristallisations et injections magmatiques syntectoniques*, and the subject-matter of *la tectonique profonde* encloses what can be observed in the deep zones where active magma interferes with orogenic deformation. Above the deep zones, in the upper parts of a structural edifice, is the region of the classic tectonics of folds, thrusts and inversions with rocks dominantly epizonal and often diaphthoretic, exhibiting deformation-effects such as mylonitization and without any important mineral regeneration or evidence of injection-phenomena. The deep zone, the lower part of the edifice, is fundamentally different—it is the domain of *la tectonique profonde* where, as we have noted, migmatic–magmatic activity interferes with dynamic action. Along certain zones the entry of magma, migma or solutions, or the transfer by diffusion of one kind or another, becomes facilitated

by the concentration of deformative effects. The products of such processes are themselves involved in movements or recrystallized or absorbed by new migmatic–magmatic importations. In the deepest zone, the orogenic structures become swamped and blotted out by granitic material, migmatic or magmatic. All signs

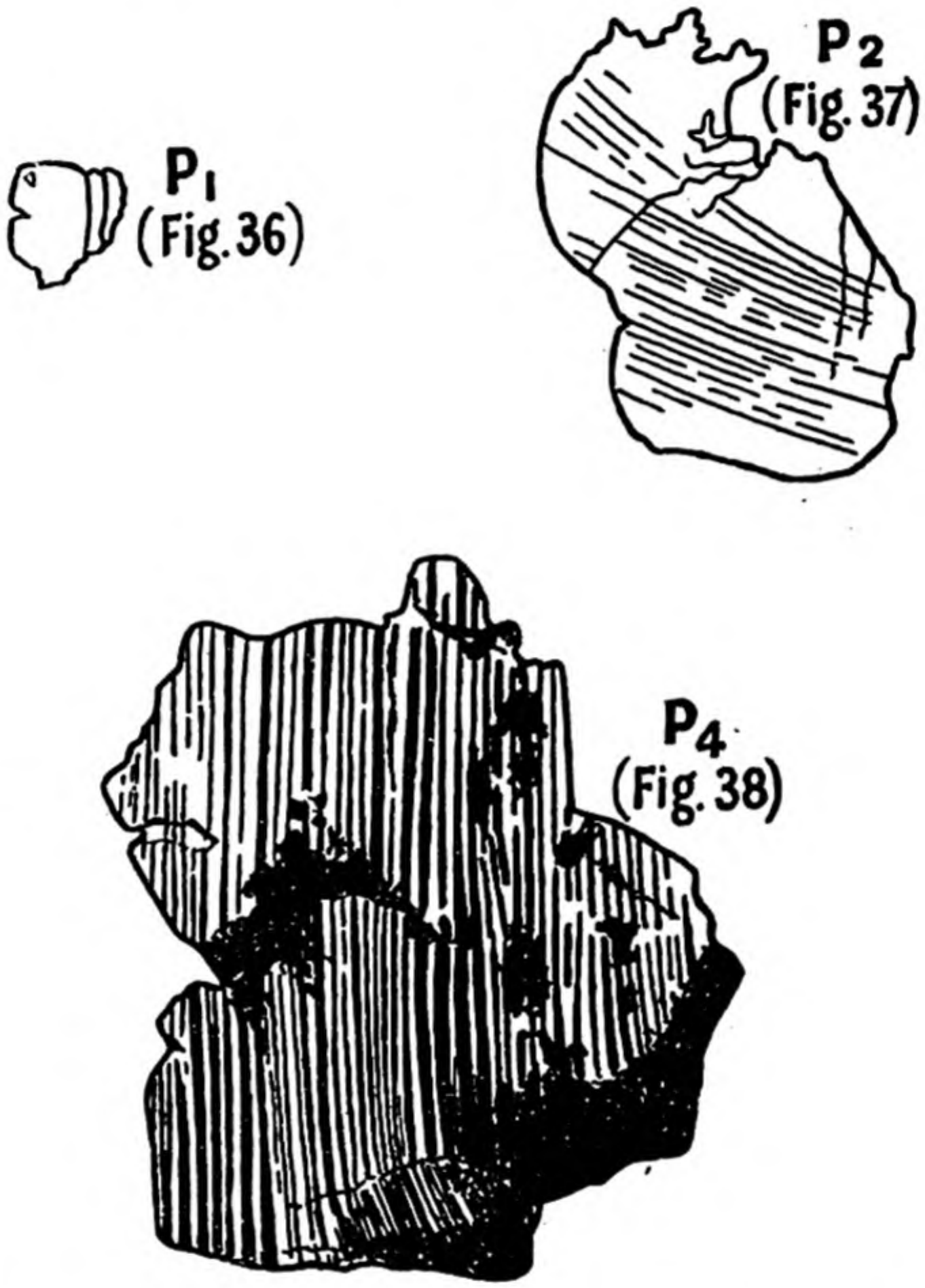


FIG. 14.—Deformed plagioclases occurring in undeformed orthoclases. (Demay, 1942, Pl. ix.)

of earlier deformations may be effaced by a final act of crystallization. By the investigation of textures—that is, by microtectonic studies—the interplay of crystallization and deformation and migmatization is established. We now look into the methods and sample the results.

The first rock I select for demonstration is an injection-gneiss from the quarry of the Roche du Loup, 1 km. south of Planfoy (Demay, 1942, pp. 77–8, Pl. ix, Fig. 36). The slice shows an

oligoclase crystal with curved twin-planes, enclosed in a large crystal of orthoclase—since the extinction of this latter crystal is uniform, the curvature of the plagioclase is to be attributed to a first phase of deformation: a narrow vein of orthoclase indicates a later phase of injection: this vein and the other crystals are deformed by a still later phase of movement—the sequence of events is deformation, injection, deformation. From a second specimen from Toissieu (Demay, 1942, pp. 77–80, Pl. ix, Figs. 37, 38), there is deduced by similar criteria a history of six events—a crystallization of oligoclase, the deformation of this oligoclase, a crystallization of a later oligoclase, the intense deformation of this, a crystallization of aggregates of muscovite and, finally, a last deformation. Both rocks display the characters of syntectonic injection. In Fig. 14, I give tracings of examples extracted from Demay's plates of felspars with curved twin lamellae, so that the scale of deformation may be assessed.

As a further illustration of Demay's methods, we may study two drawings extracted from his memoir and given in Fig. 15 here with his interpretation. The first figure shows a para-crystalline fold, swamped (*noyé*) in the crystallization, the scheme of the fold being presented in the small inset: 'on observe donc ici, en même temps qu'un plissement et des déformations orogéniques intense, une imprégnation granitique syntectonique'. In the second figure, there is recorded small-scale folding and laminage with syntectonic magmatic absorption in an anatectic injection-gneiss—'le plissement est en partie effacé par l'absorption magmatique syntectonique'.

Assembling and interpreting the microtectonic data for his type of complex, Demay (1942, pp. 129–37, 140, 170–2, 220–1; 1946*b* pp. 12–4) presents the following succession of orogenic and magmatic or crystallization phases in the history of the plutonic rocks concerned:

A. *Movements anterior to the initial metamorphism:* unimportant movements leaving the rocks still with low inclinations.
B. *Initial metamorphism and movements contemporaneous with it:* this metamorphism is independent of migma–magma and does not reach a grade higher than biotitic paragneiss or paraschist; the contemporaneous movements are tangential in style; it is possible that, after this initial metamorphism, the edifice was raised to a higher level

and at the same time the great sequence of magmatic and orogenic events began.

C. *Movements contemporaneous with basal granitization:* the complex of gneisses and mica-schists now becomes affected by tangential movements and, at its base, is absorbed syntectonically in harmony with the rise of the granite (Demay's Phase I).

D. *Décollement, gliding and laminage, under epimetamorphic conditions:* an orogenic episode during which differential movements took place (Demay's Phase II).

E. *Alkaline injections in great laccolithic zones:* the formation of various kinds of injection-gneisses and anatectic rocks (Demay's Phase III).

F. *Tangential movements later than the alkaline injection:* resulting in orthogneisses and mylonitic rocks (Demay's Phase IV).

G. *Recrystallization and magmatic injection or absorption:* a final crystallization episode (Demay's Phase V).

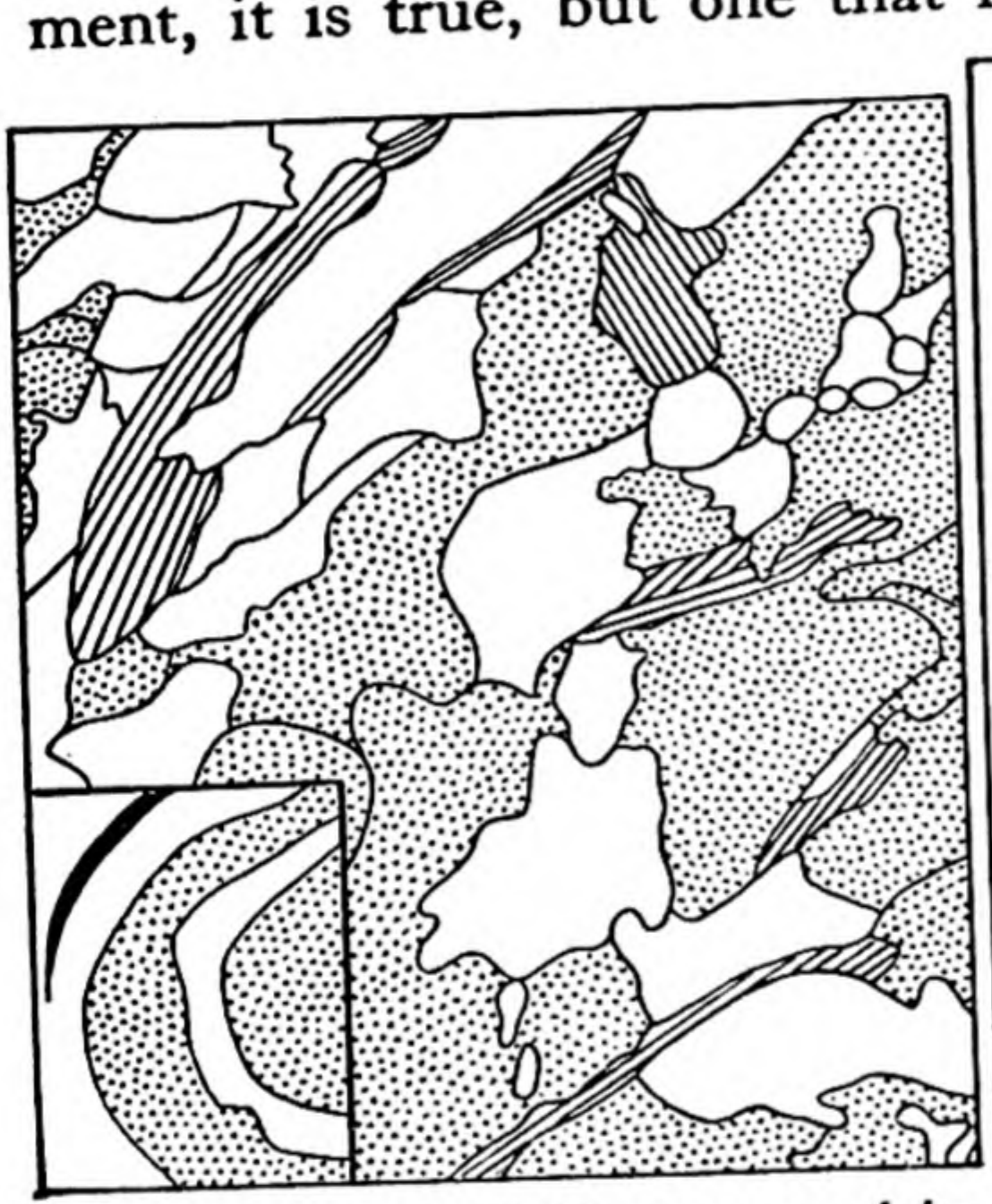
H. *Late tangential movements:* local clean-cut thrusts.

I. *Latest orogenic episode:* mostly broad folds, but some occasionally acute; straight dislocations such as the Faille d'Argentat and the pre-Stephanian *grand sillon houiller*.

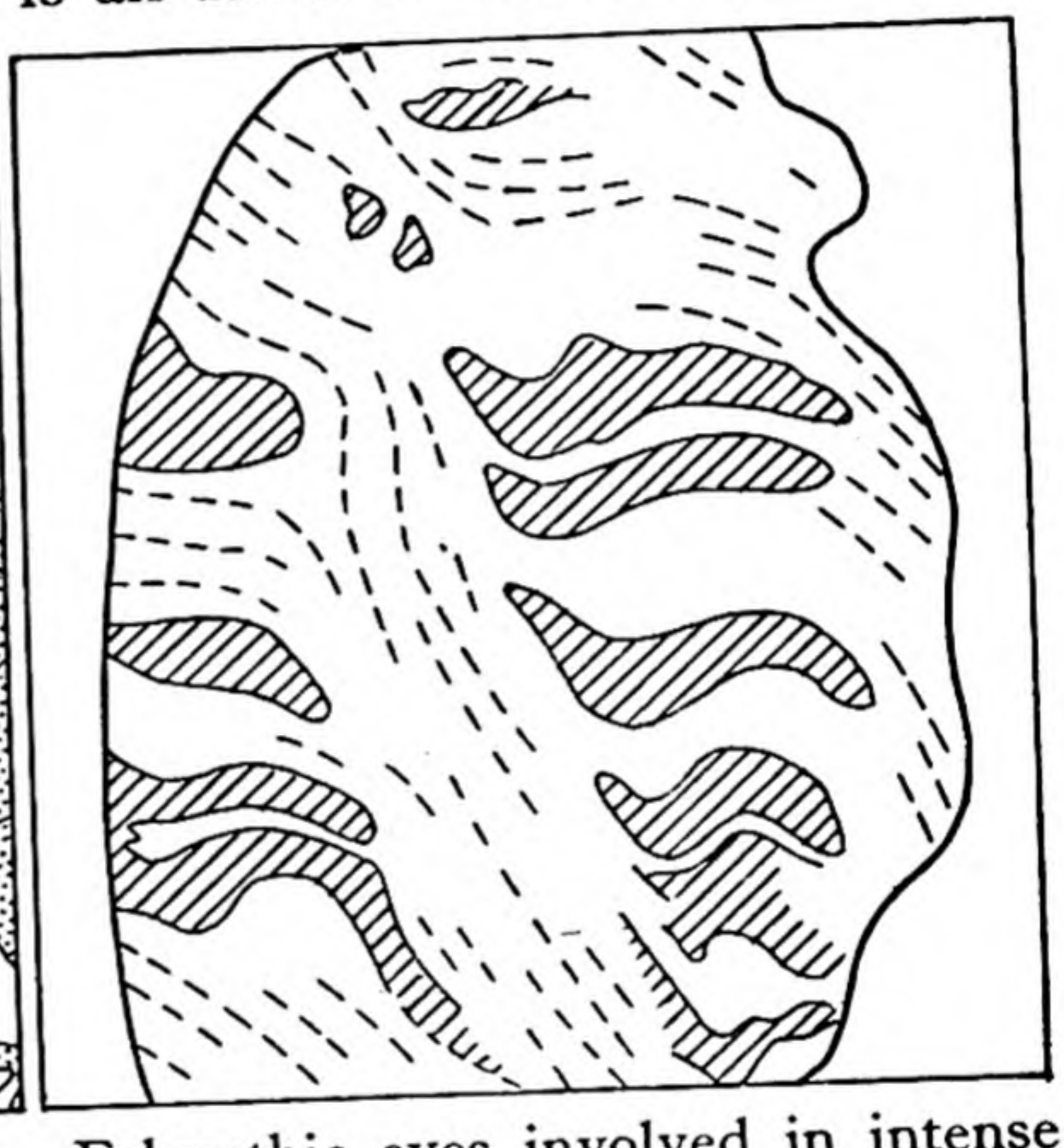
Demay considers that in certain cases his phases III, IV and V (episodes E, F and G above) constitute successive aspects of the same phenomenon rather than distinct and separate acts. When these phases are thus closely connected, they define a syntectonic magmatic injection in the strictest sense. As I have said, they have left their record especially in privileged zones which form the great zones of injection and tangential movement in the Cevennes complex. Often the complex presents an intact crystalline appearance and has a tranquil attitude, commonly nearly horizontal —the impression is given that orogenic movements have not affected the crystalline series; but the final post-tectonic crystallization has effaced all evidence of earlier movement except that presented by the *allure tangentielle* itself. The soaking outlasts or is later than the deformation.

This minute microscopic analysis of Demay's demonstrates the intricacy and diversity of the story of the crystalline schists. Like most other metamorphic geologists, I am prepared to follow Demay in a great number of his interpretations, but there are certain reservations, especially concerning emphasis, that appear to me necessary to be made. It must be admitted that by paying

attention to all the minute detail of rock-sections it is certainly possible to erect a time-sequence of an elaborate complexity. The validity of the division of such a time-sequence into a number of phases is difficult to assess, especially as the sequence is concerned with operations such as deformation, crystallization and migmatization that are notoriously local and variable in their incidence in rocks of diverse characters. A slight bending of twin-lamellae or the distortion of a mica-packet may indicate a movement, it is true, but one that is an affair of local readjustment



Para-crystalline fold, swamped in the crystallization.



Felspathic eyes involved in intense folding.

FIG. 15.—Syntectonic magmatic absorption (Demay 1942).

rather than of regional orogeny. The differing degree of diaphthoresis, crystallization or laminage in rocks of different physical and chemical characters affords to my mind no sure evidence of the existence of several phases of deformation and injection. As I have shown in Unst (Read, 1934*a*), the stability of a mineral varies with the bulk-composition of the rock in which it occurs; the determination of diaphthoresis, and especially of its domain, may accordingly be a hazardous proceeding. Arguments based on the presence of biotite and degrees of diaphthoresis are not automatically valid (cf. Demay, 1942, pp. 64–7). Demay has shown us the path to follow but we should not run riotously along it.

The relationships between granite emplacement and orogeny have, of course, been considered by many other workers in the Massif Central besides Demay. For example, Raguin (1930, 1946) has separated out, among other categories, three types of granitic intrusions, one synkinematic, a second post-tectonic but comprising mainly the batholiths that are only slightly later than the folding, and a third apotectonic, without visible relationship to any orogeny and resulting from the quiet differentiation of basic magmas such as that of the Bushveld or Sudbury. The characteristics of the synkinematic granites are of interest to us: according to Raguin (1946, pp. 25, 118), such granites form great sheets or lenticular sills concordant with the tectonic axes, and show a variable texture often oriented or gneissose, enclaves in them are suitably aligned, and mylonites, indicating a prolongation of the movement beyond the consolidation, agree in trend with the associated structures. Raguin (1946, p. 119) considers that synkinematic granites can be distinguished from injection gneisses by the scale of their development. Roques (1941, pp. 382–4, 504), in his sixfold classification of the igneous rocks of the Massif Central according to the circumstances of their emplacement, likewise assembles the criteria for the determination of syntectonic granites. Such granites occur as laccoliths and show a texture that may combine features of igneous, migmatitic and ectinitic origin and may vary from augen to fluidal, schistose to mylonitic; they are well developed on the borders of massifs where tectonic compression has operated during emplacement of the magma. This last observation raises a topic of general interest in the interpretation of textures in plutonic rocks and one we can now examine.

There can be little doubt that linear and platy textures can arise in plutonic rocks and their associates as consequences of a variety of operations which can themselves bear a variety of relations to acts of deformation or compression. To begin with, the flow of partly crystallized magma unconnected with any tectonic event gives a platy arrangement to some of the rock components that is of the same style as that presented by many bodies called syntectonic or synkinematic. The arrangement of the tabular felspars in the Drachenfels trachyte dome, for example, must be due simply to magmatic flow during crystallization; the orientation of the phenocrysts in the Land's End granite must be due to movement

of a pasty mixture of crystals and magma past its walls. If the magma is partly or almost entirely crystallized but still moving, there may result schistose, foliated or even mylonitic margins still unrelated directly to a tectonic event—unless, of course, it is held that all movement of magma has a tectonic cause. We may take as our next examples, the structures shown by the Colville batholith in the Okanogan highlands of Washington as described by Waters and Krauskopf (1941). The Colville batholith has a discordant contact and a peripheral zone of frictional breccia; no contact-metamorphic effects and no apophyses are visible; the outer zone looks like a sediment but is really a mylonitic and gneissose phase of the granodiorite body; this outer zone is followed inwards by gneissose granodiorite with swirling foliation-planes, and innermost comes a homogeneous granodiorite. These phenomena are interpreted as due to the rise of an almost solid body to its present level, the act concluding with the consolidation of the homogeneous central part—'the almost solid igneous body rose and pulverized its own nearly crystallized border' (loc. cit., p. 1405). Waters and Krauskopf suggest that movement of intrusives in the last stages of consolidation must be a common event, and that many Archean gneisses may be of similar protoclastic origin; if the granitic magma is anatectic, the deforming stresses can have the correct time-relations to give the characteristic textures of protoclasis whilst, in some cases, the high temperatures operative may erase part of the record by recrystallization.

From the Eastern States there comes a variety of observations and interpretations that are of interest here. Billings (1937, 1945) interprets the Bethlehem gneiss as a syntectonic intrusion, synchronous with folding, because it shows a flow-structure giving a primary foliation concordant with that of the country-rocks which wrap around the gneiss body; the operation of emplacement is one of 'forceful injection'. Billings (1928) had earlier separated *synchronous* from *subsequent* batholiths, the former class having characters associated with orogeny, regional metamorphism and migmatization—characters that Browne (1931) later codified. Balk and Barth (1936, p. 832) are of the opinion that for the production of the Dutchess County series of metamorphic and migmatic rocks it is essential that folding and shearing of the sediments had gone on during a magmatic soaking from syntectonic

intrusions; if the magma had entered afterwards, typical contact-metamorphism would have been produced. The Dutchess County operation is the syntectonic imbibition of Seng (1934). Barbour (1930) has shown that the 'igneous invasion' that produced the Bedford augen-gneiss came after a period of dynamic stress, the medium being soft enough to allow the felspars to take on a more or less idiomorphic form. Currier (1947) considers that the Chelmsford granite is of replacement origin and that its banded and layered structures are inherited from an original sediment.

In all the examples just presented, there is a certain similarity in the critical textures and structures, in spite of the diversity of the time-relations between deformation and magmatic or migmatic activity. It is clear that the interpretation of such textures as augen-texture, foliation, fluxion-texture and similar alignments and platinesses can be a difficult matter leading to diversity of opinion. As an illustration we may consider the different views on the interplay of intrusion and metamorphism in the Altvatergebirge held by Becke, Kölbl and Bederke (see Erdmannsdörffer 1936). Here the core of granitic gneiss is interpreted as follows: by Becke as a protoclastically deformed intrusive, by Kölbl as a mechanically deformed solid granite with a later epizonal deformation, by Bederke in 1925 as a syntectonic granite and in 1935 as an old granite intrusion that has suffered diaphthoresis in one phase of the Hercynian orogeny and static recrystallization in a later phase.

The syntectonic character of intrusion or soaking is not to be decided simply on the form and structure of the body or of the imbibition zone, but by a series of detailed observations leading often to difficult deductions. The significance of an augen-texture may be revealed only after detailed field and laboratory studies. Suess (1937, pp. 8, 40, 60) has remarked on the change of opinion brought about by the growing realization of the importance of *Sammelkristallisation* and diffusion in producing textures and of the dependence of the visible 'flow-textures' on orogenic deformation. He emphasizes the fact that many celebrated augen-gneisses, such as the Bittesch gneiss of the Moravicum, the Red Gneiss of the Erzgebirge and the Central Gneisses of the Alps, are not syntectonic wellings-up from the geosynclinal depths into the fold-structure but are passive bodies deformed by the orogeny.

Magma-movement can occur during orogeny, but it does not belong to the essence of the operation.

In the foregoing pages of this section of these remarks, I have dealt chiefly with the possible syntectonic character of magma or of migma and only incidentally with that of the more diffuse materials resulting in permeation, imbibition or soaking. The time-relations of soaking are in many terrains more important than those of magma-emplacement. From many possible examples, I select two. In his dissection of the time-relations between deformation and crystallization in the katarocks of the Austrian Waldviertel to which reference has already been made, Waldmann (1927) concludes that the celebrated Gföhl gneiss is due to the regional permeation of alkali solutions simultaneously with strong movement. In the gneiss are thin, fine-grained bands, interpreted as katamylonite, that pass into coarse-grained gneiss with stronger impregnation with potash-felspar and progressive *Sammelkristallisation* of the comminuted sillimanite, biotite and quartz. For my second example, I reintroduce the problems of the albite-gneisses of the Dalradian to which I made reference last year (Read 1948*b*, p. 181; this volume, p. 232) and aspects of which I have considered in earlier pages of this present address.

As I displayed last year, the discussion of the origin of the albite-gneisses of the Scottish and Irish Dalradian is concerned with soda-transfer and, consequently, with time-relations of various kinds, as I have already shown in these pages. D. L. Reynolds (1942) has demonstrated on petrographical and chemical grounds that the albite-gneisses are not sediments crystallized without change of composition, but result from an introduction of sodium silicate and a consequent expulsion of other constituents. As I have already mentioned, the formation of the albite is later than that of the biotite, this being contemporaneous with deformation. In her discussion of the relations of the albite-gneisses to orogeny, Reynolds makes comparison between the Dalradian phenomena and similar occurrences in the Caledonian fold-belt of Norway. For both regions, there is an assemblage of recumbent folds, 'inverted' metamorphic zones and migmatites, the latter representing a syntectonic igneous phase. The albite-gneisses owe their existence to the driving forward of sodium silicate in advance of a

syntectonic igneous mass emplaced along the axial belt of a recumbent anticline. (The reason why anything should prefer to move forward along such a belt needs study.) North-east along the strike of the same structure there appear the migmatites of Aberdeenshire. In Fig. 16 there is given a diagrammatic section showing the connexion between migmatites and albite-gneisses postulated by Miss Reynolds. I do not wish to depart far from the assessment made in an earlier page of the validity of generalizations on Highland metamorphic and plutonic history, but nevertheless I find Dr. Reynolds's propositions very attractive. They bring all the plutonic elements into the story and point the way for

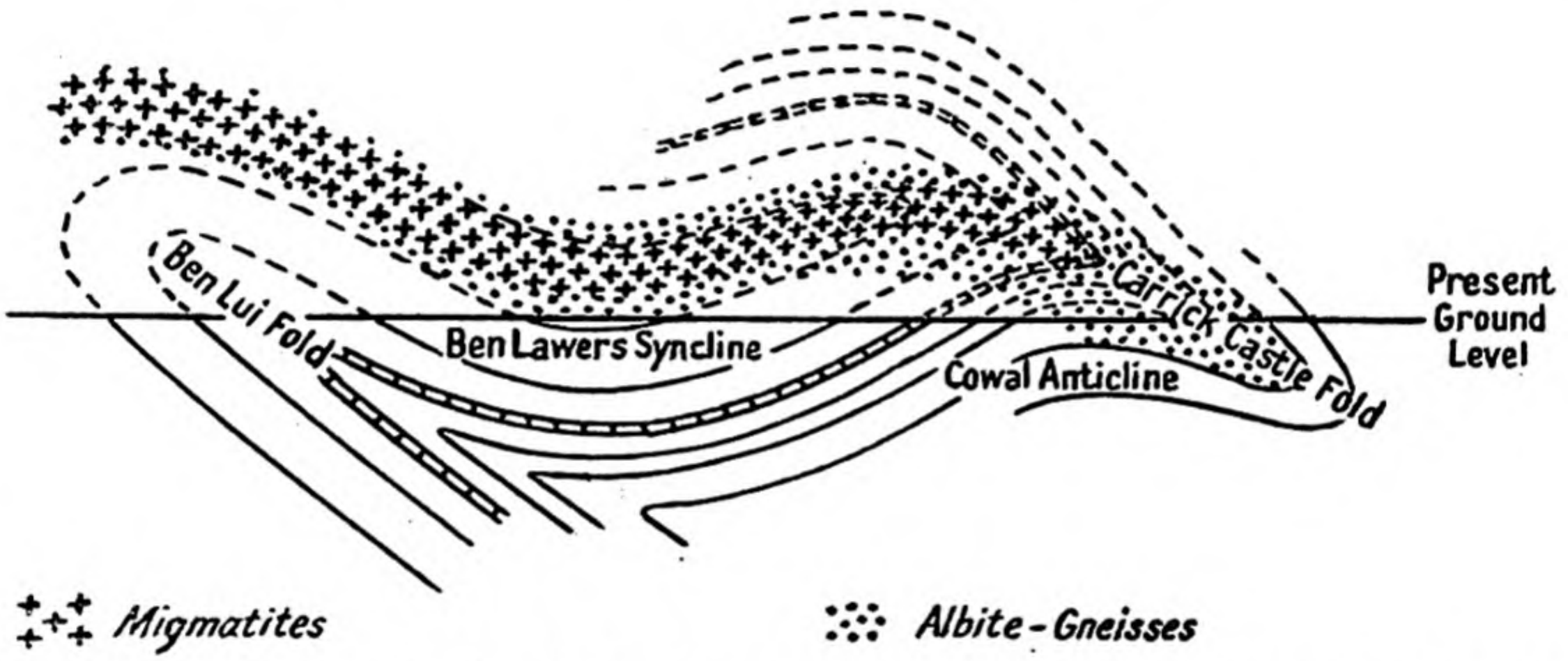


FIG. 16.—The relationship of migmatites to albite-gneisses in the recumbent folds of the South-East Highlands. (Reynolds, 1942.)

further work; they will prove more fruitful than those proposals that keep migma–magma out of the metamorphic history of the Dalradian rocks.

Somewhat similar views concerning the relations between recumbent folding and migma–magma invasion have been expressed by Demay (1946*c*) for the Margeride nappe. The injection-gneisses of the Bessenoits may be attributed to a laccolithic zone of injection, the advance guard of the deep injection-zones of the Rouergue, evidently moved into place 'à la faveur du charriage de la nappe', but of a less distant origin. It is as well to recall, however, that Suess (1937, pp. 17–9) is a critic not only, as we have already seen, of syntectonic intrusion in general but of syntectonic albitization in particular. For the Alpine occurrences, he holds that

there is no relationship between albitization and the limits of the great tectonic units; albitization is found in the pennid *Schieferhülle* of the Tauern, in the grisonid mica-schists of the Schneeberg, in the middle east-alpine gneisses of the Wechsels and especially in the Ötztal mass and the Koralp: its domain cuts across the mountains with irregular margins and is unrelated to any visible magmatic rocks. Albitization is to be considered as a separate operation independent of any previous metamorphism—'in jeder Art des metamorphen oder nicht metamorphen Zustandes in jedem Gesteine von entsprechendem Stoffgehalte und bei entsprechender mechanischer Beanspruchung in Erscheinung treten kann'.

In an earlier page, I have given in Table D the contents of the syntectonic phase of the Variscan magmatic–orogenic cycle in the Thuringia-Saxony block as this is seen by Scheumann. Among the rocks of this phase there appear prominently granulites of various kinds and, accordingly, it will not be out of place to examine the proposal that granulites are syntectonic magmatic rocks and, at the same time, the many other proposals concerning this classic and enigmatic group. Here I use the term granulite in the sense in which it is employed by Saxon and Austrian geologists and not by those working in the Scottish Highlands.

We may start the discussion with the consideration of the Waldviertel granulites of the Moldanubicum whose genesis has been dealt with by Suess (especially 1926, pp. 79–96, 234; 1937, pp. 27–30). The classic explanation of Credner, that granulites in general are simply granites that have acquired a fluidal texture during crystallization, is not favoured by most Austrian observers. It is true that Limbrock recorded what he considered to be the injection of granulite along the schistosity-planes of biotitic hornfelses and the inclusion of xenoliths in the granulite body and, on this evidence, concluded that granulite was an intrusive rock in which no essential post-magmatic crystallization had taken place. The opposite pole of opinion is represented by the views of Waldmann (e.g. 1927), Kölbl, Köhler and others; the Moldanubian granulites are mixed rocks resulting from a copious magmatic soak coming from the depths and replacing considerable portions of the sedimentogeneous country-rock—the garnets of the original rocks have kept their places but the biotite- and plagioclase-rich groundmass between them has been replaced by introduced

potash-rich material. Seng (e.g. 1931) demonstrates how different the granulitic texture is from that of igneous rocks and maintains that the granulites show all the textural characteristics of para-crystalline kata-metamorphic rocks. Suess remarks that the granulites have the textures, mineral constituents, banding, grain-size and other characters common to members of the Moldanubian series. These include the Gföhl gneiss and many definitely sedimentogeneous types; all the members present the history that makes the Moldanubicum a crystalline and tectonic unity and the granulites are crystalline schists of the same zone and facies as the associated metamorphosed sediments. In these latter, all the original sedimentary textures and, in the granulites, all the original igneous textures, have vanished. The granulites are neither sheared solidified igneous rocks nor syntectonic magmatic bodies—like the rest of the Moldanubicum, they are true crystalline schists of the eclogite facies of which they are the acid members. The interpretation of the Saxon granulites is, rightly, bound up with the interpretation of the much-disputed tectonics of the Granulit-Gebirge. According to Suess, these lie like a foreign body on a non-metamorphic fold-belt; the Granulit-Gebirge is a block with katametamorphic post-tectonic crystallization but having thrust margins that have suffered paratectonic metamorphism—the facies is that of the Moldanubicum and the genesis is the same.

I do not propose either here or anywhere else to enter into a discussion of the plutonic history of the Granulit-Gebirge, and content myself by reaffirming my belief that, as in all terrains of crystalline schists, light will come when time goes into the geological map, when textural studies indicate a plutonic history that can be related to place on the ground.

VIII. THE GRANITE SERIES

I now come to the heart of the matters that have had my attention, either latent or manifest, throughout the half-dozen presidential addresses that it has fallen to my lot to make. Today and on earlier occasions, I have built up on diverse scales the history of the plutonic rocks. I have brought migma–magma into the story, have related granitic events to stages of orogeny and have

made references to granite of various kinds in their varied settings. I have now to display the Granite Series (Read, 1948*a*; this volume, p. 192), which, as I said last year, 'attempts to relate the plutonic phenomena at the various levels of exposure and to give a unity to the processes of granitization, migmatization and metamorphism at depth and at successively higher positions and at later times'.

For more than a century there has been a recurrent idea, especially among the French, that granites were of at least two kinds. In 1847, Élie de Beaumont accepted Virlet d'Aoust's proposal that some granites are of metamorphic origin and distinguished 'very clearly between granites with chilled margins and but feeble contact effects and those surrounded by veins and stockworks. The first type evidently possessed *la vertue métamorphisatrice* to only a small degree and lost at their contacts the faculty of granitoid crystallization—"ils étaient, pour ainsi dire, éventés".' Fournet likewise distinguished between eruptive porphyries and metamorphic porphyries. But the French saw no necessity to make a genetic separation between the different kinds of granite. Thus, in 1869 Delesse held that metamorphic granites could be mobilized and driven by orogenic forces into higher levels of the crust, there to appear as cross-cutting bodies that had left all evidences of their real origin behind in the depths. There was no objection to one unified granite-making act: that is, to a Granite Series. In the eighteen-seventies, what I have called a Lake District School maintained similar opinions in England. Clifton Ward emphasized that 'it is well to remember that a granitic mass formed by metamorphism of rocks *in situ* is almost sure to become an intrusive mass at *some part*', and A. H. Green, believing as I do that there were 'granites and granites', considered that advancing stages in the process of metamorphism of rocks *in situ* could give rise to three forms of granite—bedded, amorphous and intrusive; the irruptive behaviour of the last-named types being reasonably 'attributed to an increased degree of energy in the metamorphic process which gives rise to it'. I have elsewhere presented in detail these early statements of a Granite Series (Read, 1943, pp. 70, 71; this volume, pp. 57–8; 1944, pp. 51–4, 88; this volume, pp. 97–103, 161).

French thought continued to develop along the national lines

and in directions of fundamental importance for the Granite Series. Michel-Lévy and Lacroix codified the different kinds of granite contacts, showed that felspathized zones around granites increased with depth and suggested that contact metamorphism at higher levels becomes confluent in the depths with regional metamorphism. Termier held that the production of granite was an episode in regional metamorphism; if a granite is surrounded by a vast aureole of metamorphic rocks, it has certainly been formed in place, by complete fusion of a eutectic, whilst the neighbouring rocks were regionally metamorphosed; if, on the other hand, a granite has only a narrow aureole, it has certainly come from somewhere else, ready made. (For details of the views of Michel-Lévy, Lacroix and Termier, see Read, 1940, p. 226; this volume, pp. 4–6; 1944, pp. 55–60; this volume, pp. 103–12). I have already acclaimed, almost lyrically, the beauty and fruitfulness of this classic French concept of the relations between depth and character of granite contacts (Read, 1944, pp. 60–1; this volume, pp. 112–3). The concept becomes yet more splendid when, as we see later, time is applied to it.

Many of the modern French workers have emphasized, perhaps unduly, the historic twofold division of granites. Demay (1942, pp. 197–8) for a score of years has distinguished intrusive granites from deep granites. Intrusive granites are accompanied by contact metamorphism or, at the best, very limited injection-phenomena; deep granites—they need not be very deep, but are deeper than the intrusive variety—are bound up with the crystalline schists and their emplacement or formation is accompanied by impregnation and injection; transitional types are, of course, admitted. The deep granites do not form isolated bodies and are revealed by erosion on axial culminations; they constitute a magmatic zone of migmatic granites and true granites (*granites francs*) below the zone of regional metamorphism. In general, the intrusive granites are post-tectonic, the deep granites are mainly syntectonic, but there is a complete series of magmatic operations before, during and after orogeny, 'depuis les venues les plus anciennes jusqu'aux intrusions granitiques tardives dans la couverture sédimentaire déjà plissée et cassée' (Demay, 1942, p. 198). Raguin, again, in his *Géologie du Granite* (1946) erects two great categories, *les granites d'anatexie* and *les granites en massifs circonscrits;* the

first category is of those vast diffuse granites which impregnate immense segments of the crust, are associated with migmatites and are the result of anatexis; the second category is of granites that have sharp limits attended by narrow contact-zones, are homogeneous and appear to be replacements or displacements of their country-rock—they occur as batholiths or intrusive sheets. Raguin (1946, p. 29) admits the existence of intermediate forms, as, for example, those congeneric with migmatites. The ascent of granite in general is syntectonic or post-tectonic (1946, p. 111).

Jung and Roques have elaborated this twofold classification and have related in some measure the form and composition of granites with time. In their early summary of the crystalline schists of the Massif Central, Jung and Roques (1938; Roques, 1941, p. 42) deal with the granites that are *congeneric* or genetically associated with the crystalline schists. It is instructive to follow the sequence of forms in their presentation. To begin with is granite in autochthonous *anatectic batholiths*, formed in place at the base of the migmatite complex, variable and nebulitic in texture, possessing hazy margins, carrying garnet and cordierite and building a vast batholith in the eastern part of the massif. Next come the *subautochthonous batholiths*, closely associated with the first type, but occurring along the axes of maximum migmatization and definitely cutting the country-rocks; such batholiths have risen slightly from their anatectic roots by digestion of their roof and can be exemplified by the Granite du Guéret occurring in the midst of the cordierite-bearing anatectic Gneiss d'Aubusson. *Normal or intrusive batholiths* are plainly intrusive and cross-cutting—the granite has risen and lost contact with its original surroundings, as in the Margeride, Mont Lozère and elsewhere. As a second series, Jung and Roques separate off the *granites extravasés* that have known walls and roofs and comprise discordant chonoliths, concordant laccoliths and synkinematic injections.

For the south-western part of the Massif Central, Roques (1941, pp. 382–5) has elaborated the characters and time-relations of the relevant eruptive rocks. These comprise four different types. Granites with a synkinematic or synectinitic texture revealed as an augen, fluidal, schistose or even mylonitic character occur as large laccoliths situated above the migmatite front; they are contemporaneous with the end of the general metamorphism, as are the

migmatites themselves. A second type of granites forms small massifs, whose intrusion has been guided by an obscure fracture system in the migmatites; such chonolithic granites were emplaced shortly after the general metamorphic epoch. Much later granites are represented by the Millevaches two-mica granite, independent of the metamorphic zones that they traverse, later than the essential tectonic phases, but, maybe, contemporaneous with a late phase of folding; their texture has nothing in common with that of the ectinites, in which they produce a narrow zone of thermal metamorphism. Finally, intrusive granite batholiths are not related to the migmatites and they cut indifferently all the metamorphic zones; they are homogeneous, often porphyritic rocks, in sharply defined massifs, and their intrusion is clearly later than metamorphism or folding. We see here once again an example of change of type and form of granite bodies with time.

One of Roques's intrusive granite batholiths is that of Sidobre in the Montagne Noire. The Sidobre granite is an example of a high-level granite, intrusive into scarcely metamorphosed rocks, and presenting a sharp contact and a well-developed thermal aureole; its intrusion was later than the Hercynian folding, post-Viséan and pre-Stephanian (Roques, 1941, pp. 82, 94, 108). Granites of this late group are chemically consanguineous with the migmatites and anatectic granites into which some of them are intruded. Roques (1941, p. 425) suggests that such late granites, apparently completely independent of the migmatites in manner of occurrence and in age, may yet be derived from a deep zone of anatexis and may have thence risen to a zone entirely non-metamorphic in character. This suggestion is a logical development of the sequence of forms presented by Jung and Roques and outlined in the previous paragraph of these remarks, but, as we see later, it has its difficulties. It is of interest to recall that many years ago Koenigsberger (1912*b*) held that younger granites are simply a quicker rising portion of the same magma whose more lengthy injection in the depths was responsible for the associated gneisses; thus, he interpreted the cross-cutting Younger Granite of Balmoral in the Highlands as genetically related to the diffuse Older Granites of the same region. The younger, cross-cutting granites are, of course, the diapir granites of Wegmann.

As we have seen, the contrast between deep and shallow granites,

metamorphic and intrusive granites, migmatic and magmatic granites, diffuse and circumscribed granites, has always been apparent to field-workers. It is the basis of such classifications as those of gneiss-massives and granite-stocks (e.g. Lepsius, 1912; Berg, 1913), of discordant and concordant batholiths (e.g. Geijer, 1916); of synchronous and subsequent batholiths (e.g. Billings, 1928; Browne, 1931) of the Suess batholiths and Daly batholiths of A. M. MacGregor, of the Kerngneisbatholith and the pluton (Scheumann, 1932). Recently, this contrast has entered into the discussion of ore genesis and granitization given by Sullivan (1948), who emphasizes the economic importance of distinguishing between synchronous and subsequent batholiths. Synchronous batholiths are a product of granitization and their rock has not reached a magmatic stage; accordingly, elements, present in the rock undergoing granitization, that cannot be accommodated in the lattices of the granitic minerals being formed become concentrated towards the margins of the granitization area, there to appear as ore-deposits.

In the catalogue of classifications that I have just listed, batholith appears to be a term applied to bodies of differing shapes occurring at different crustal levels. The time seems to be upon us when the only requirement of a batholith is a large size. It would be better if we admitted this and agreed with Loewinson-Lessing (1936) and Currier (1947, p. 82), for example, that the postulated shape of batholiths is a mere supposition.

Taken at their face value, the twofold classifications of granite masses appear rigid and secure, but a few reflections show that this is by no means the case. As I have noted, the French workers admit a variety of transitional types, such as is provided by certain of the granites congeneric with the crystalline schists. Further, there are many cases, in which one and the same granite body shows in different parts the characteristics of both types; as Ward said, a granitic mass formed by metamorphism *in situ* is almost sure to become an intrusive mass at some part. Suess (1926, pp. 25–6) has emphasized the variation in marginal phenomena shown by many of the Hercynian granites of central Europe. It is well to realize, he says, that the contact phenomena at the base of a great magma-body are quite different from those at its roof. At the roof, the rising batholith has lost its energy and has come to

rest; it is able to supply only a small amount of heat to the country-rocks and the pressure is not great. At the floor, the magma-body is able to give its great excess of heat to the country-rocks, to permeate them with mineralizers under high pressure and for long duration—there is thus sufficient time for far-reaching crystallization and transformation of a kind quite different from that of the roof. Ebert (1935; Suess, 1937, p. 56) has shown that the great Lausitz mass has an outer side of cordierite-hornfels produced by the thermal metamorphism of Palaeozoic sediments and an inner side of venites and migmatites. Wegmann (1938) has contrasted the intrusive and the impregnation contacts of the Julienhaab granite of Southern Greenland. The Killiney schists of the Leinster granite contact or the sillimanite-gneisses of the Cairnsmore of Fleet contact provide as good, though smaller, samples of variation.

The duality of granite occurrences disappears so soon as we add time to the form considerations. Classifications on form and on contact phenomena have their uses, but with the entry of time the classifications fade away and there appears the unified sequence of plutonic events linked up with place that I call the Granite Series. We have seen the beginnings of the erection of a Granite Series in the work of Delesse, Roques and many another. There are a number of statements, inquiries and discussions now to be made that will help to complete the edifice. I assume, perhaps rashly, that all reasonable persons admit, as our forerunners did, the reality of the two end-members of the Granite Series—granitization granites and cross-cutting granites. Granitization needs no longer to be argued and cross-cutting never has been, no matter what great problems its existence raises for the inquisitive. Our end-terms are secure, granitization granites commonly invaded by cross-cutters.

When we consider the expression of the first term, an *area of granitization*, it is not incumbent on us to demonstrate any particular mechanism for the process, whether migmatic or magmatic, wet or dry, gross or atomic, indigenous or introduced. It is sufficient to invoke a state of extreme chemical mobility during which arises the plutonic series of granites-migmatites-metamorphites. Autochthonous granites are associated with a galaxy of migmatitic types which pass into the more or less orderly succession of the regionally metamorphosed rocks. We require only the broad

picture and can now dispense with the small time-differences that have had our attention in earlier pages and on previous occasions.

The *granitized material begins to move.* There are countless possibilities in such movement. It is likely that during granitization the plutonic segment may move as a whole, but as the process advances some portions may move relatively faster than others. But still the migmatite-metamorphite envelope may remain substantially in contact with its granitized core and a complex sequence of minor events involving crystallization and movement may be decipherable in both envelope and core. Chemical mobility continues more important than mechanical mobility.

Sooner or later the core begins to part company with its envelope—*true mechanical mobilization has begun.* This is an operation that I have discussed at length in another place (Read, 1944, pp. 87–90; this volume, pp. 157–61). With mechanical movement, intrusion takes place; as I have before remarked, however, such intrusion need not necessarily be of igneous material but may vary from the nebulous migma to the equally nebulous magma. Granitized material, rendered mobile by soaking, will tend to move as a whole upwards into regions of lower pressure or it may be pressed forward into structural domains of a similar kind. If the plutonic segment is subjected to non-uniform pressure, some kind of straining mechanism may come into operation and what becomes moved from the granitization centre may be but a fraction of the granitized material; the mobilized material may thus differ in composition from the granitized core-rocks and may itself change in character as it continues to move into a different environment. Further, in a varied series undergoing granitization, the development of the process will depend in some measure on the composition, structure and texture of individual rock types; it is a common observation that extreme rocks such as epidiorites, limestones and quartzites resist migmatization. It is possible, therefore, that mobilization may be selective; Backlund (1943, pp. 83, 121) holds that, since pelitic rocks are granitized and mobilized before quartzite and limestone, their products may appear as intrusive bodies in their more resistant associates.

In the early stages of the *movement of migma*, there may be a tendency for the envelope to become only locally disconnected from the mobilized body but, by straining off and by the

concentration of fluids, emanations, volatiles and the like under lowered pressures, the migma may change its character and become *magma independent* of its plutonic surroundings and move into higher levels of the crust. On consolidation, such migma–magma bodies will appear transitional between the subautochthonous masses and the true cross-cutters; their contacts may vary from felspathized zones of imbibition to aureoles of plain thermal metamorphism; they themselves may present variable migmatitic portions and more homogeneous magmatic portions.

Finally, the migma–magma may free itself completely from its plutonic associates and move high in the crust, even into non-plutonic regions, to appear there as the intrusive *cross-cutting diapir granites, en massifs circonscrits*. Since they are mechanically mobilized, their margins will be sharp and most of them will be attended by conspicuous aureoles of thermal type. Their emplacement is often an affair of structural adjustment such as ring-faulting, laccolithic doming or plain 'pushing and shoving' along weak belts; their rocks, however, may yet reveal an ultimate granitization origin by characters of mineral composition or texture. Such granite bodies, formed from a semi-crystallized viscous migma–magma moving past mostly stationary walls, provide the field for *Granit-tektonik;* they make most of the plutons of Hans Cloos that sit between the regional structures and not in them. Some of them provide well-developed protoclastic features. The mechanism of emplacement of these circumscribed bodies is more often to be determined from the structures of their country-rock walls than from their own. Evidences of drag and movement of the wall-rocks, combined with that presented by the metamorphic history of the aureole-rocks, may be decisive.

Before we can display the Granite Series in its full shape there are certain interesting matters to be discussed. Two are concerned with what I propose to call *overtaking*. Granites may reasonably be formed and mobilized at various intervals during a plutonic act. As Wegmann (1935*a*, p. 335) has argued, an early mobilized and solidified granite may be overtaken by the main granitization of the same sequence; Early Granites appear cut by Main Granites, Main Granites by Late, but all are of the same blood. As I have already noted, the field-worker determines the dates of the final acts of consolidation of granites and such dates may be of little

value for correlation purposes. This aspect has been considered by von Eckermann (1936, p. 336) in connexion with magmatic granites. He says, 'The age of a granite is a relative conception associated with a comparatively shallow erosion section of the earth-crust. We have no guarantee as to which of two magmas—the one intruding the other—is really the younger one. The invaded magma may have reached the level at which it is uncovered today later than the invading one, which may nevertheless have been differentiated at an earlier date. One and the same magma may even have been split into two or more separated magma portions displaced laterally and each working its own way at different accelerations to higher levels.' Von Eckermann considers that magmas which appear to belong to different orogenic periods may actually be genetically contemporaneous, and concludes that much of the classic granite chronology is 'based more on intuition than on actual facts'. These are important conclusions to which I return immediately after we have considered the change of composition of granitic material during ascent in the crust.

This is a question that can be treated only in very general terms at this late stage of these proceedings. In such terms, it will be agreed that autochthonous and migmatitic granites are soda-dominant types, often granodioritic in character and with abundant oligoclase, whereas later granites of the same cycle are potash-dominant, with microcline in abundance. A few samples will illustrate. Drescher (1926) long ago compared the soda-dominant 'injection-rocks' with the potash-dominant intrusives in the Friedeberg mass; in Orijärvi, Eskola (1914) showed that the oligoclase-granite associated with plutometamorphism was followed by the microcline-granites of the coast type; Niggli (1946) has summarized the Finnish evidence—in Pellinge, granites show increasing alkalinity with time and vary from granodioritic to aplitic-granitic, and in other classic Finnish areas the same change is seen. This general rule has been elaborated by Backlund (1943) with results that are of fundamental significance. The oldest Urgranites, resulting from a reconstructive homogenization, are granodioritic in cast, their detailed composition varying with the replaced country-rocks; if time-differences can be demonstrated in the Urgranites, the 'younger' parts are richer in potash due to differential mobilization. The Younger Granites, especially

abundant at higher erosion levels, have a position intermediate between batholiths and the large Urgranite masses to which they are often marginal; they occur as concordant sills in anticlinal structures, discordant stocks in synclinal structures and are mineralogically characterized by potash-felspar and muscovite—*k* for the Urgranites is 0·42, for the Younger Granites is 0·62. When the Younger Granites form stronger mobilized 'intrusions' in the higher levels their migmatitic aspect is much reduced. Later granites, such as the Rapakivi, are still richer in potash.

Backlund proceeds to use this chemical-age difference to determine the significance of conglomerates in the Pre-Cambrian. Suppose the conglomerate to carry boulders of Urgranite type, say granodioritic in character, and in the same ground a younger granite, also granodioritic, cuts the conglomerate and superincumbent beds—the conglomerate is then truly basal and separates two tectonic units of different ages. If, however, the younger granite is of Younger Granite type, rich in potash, then the age differences are much less and the conglomerate of no great tectonic significance. On quite different grounds, von Eckermann (1936, p. 336) admits that two granites bearing the same relationship to a basal conglomerate, one 'younger' and one 'older', may yet be genetically the same—their different rates of ascent accounting for their different times of appearance on the scene. Like Backlund, he questions the validity of the classic granitic sequences of Finland employed in the division of the Pre-Cambrian of that country. Lastly, if the *k*-value is to be used as an index of time in the Granite Series, then we must query Roques's interpretation of the Sidobre granite as mobilized migmatitic granite, already referred to, as this interpretation is based upon chemical similarity —and not on chemical differences.

We are now able to view the Granite Series, linking granites in time, place and character. Place may vary vertically or horizontally, time differences may be small or immense, characters may be seemingly inconstant, but, nevertheless, I reassert my old view (Read, 1944, p. 90; this volume, p. 162) that 'though there may be granites and granites, most of them are of one kind and all of them may likely be of one connected origin'. Perhaps I should hasten to remind myself that if, as the magmatists hold, each granite is related to a basic magma and its formation is thus a separate

event, then the Granite Series has no validity whatever. On grounds that I need not repeat here, I prefer to consider granites to be for the most part genetically unconnected with basic magma. I set out a skeleton of the Granite Series in Fig. 17.

As an example of the Granite Series I present the Hercynian belt of central and western Europe, the geology of which has been summarized by von Bubnoff (1930) and Suess (1926, 1937, 1938*b*).

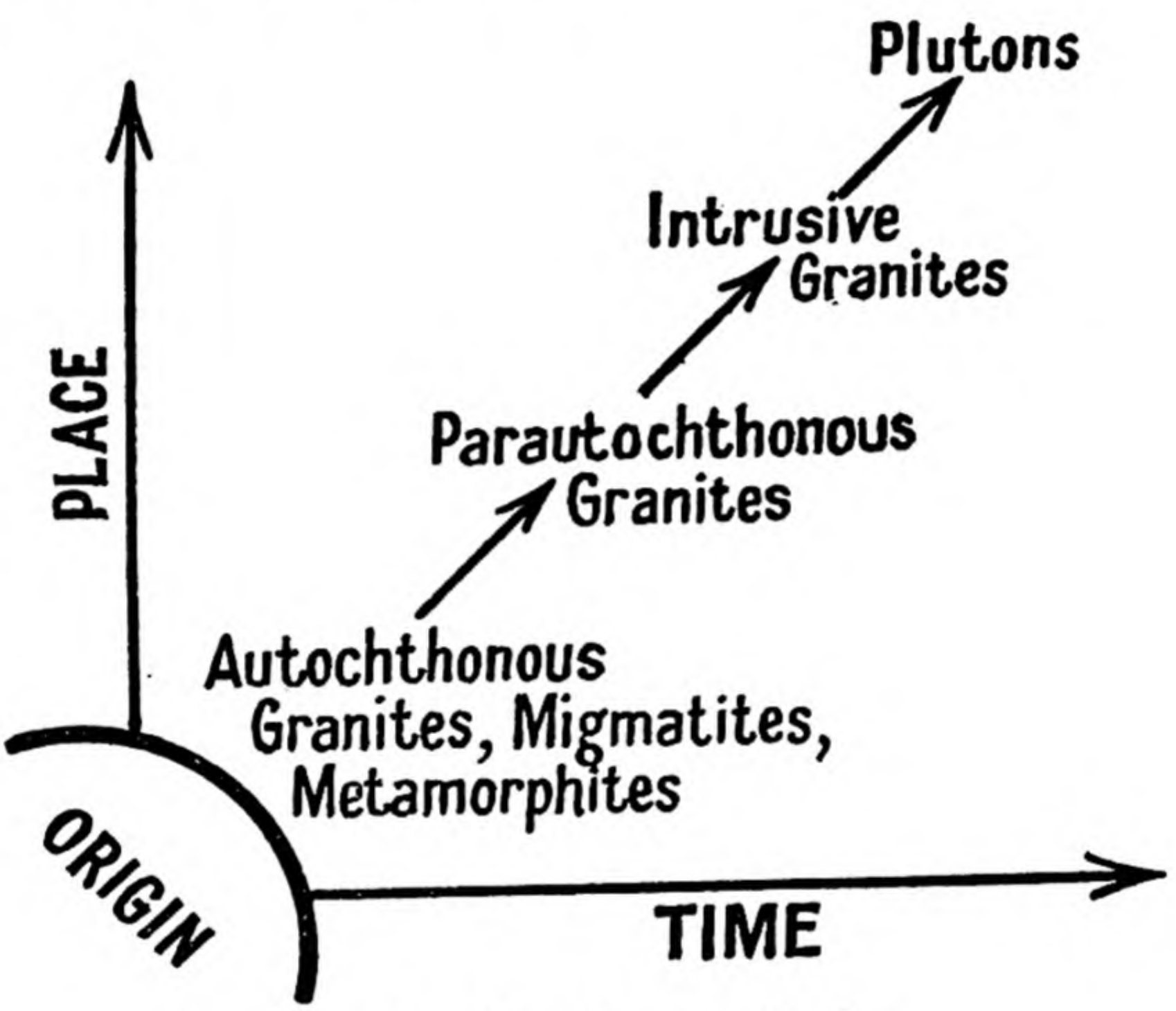


FIG. 17.—The Granite Series.

A detailed time-and-place study of the granites of this belt would undoubtedly reveal general principles of great interest and importance; lack of time—and lack of knowledge—prevents me giving anything more than a few results here. Suess (1926, pp. 9, 42, 237–44; 1937, pp. 58, 67) has argued that the countless manifestations of granite in the Hercynian belt of central Europe—granites whose formation overlaps the orogeny at both ends and whose ages may spread from pre-Cambrian to post-Culm—are all blood-relations. Rise of the magma lasts longer than the tectonic events and many granites break through the structures and appear to be post-orogenic. As Bucher (1933, p. 289) has remarked, 'granitic magmas had accumulated in sufficient size to rise

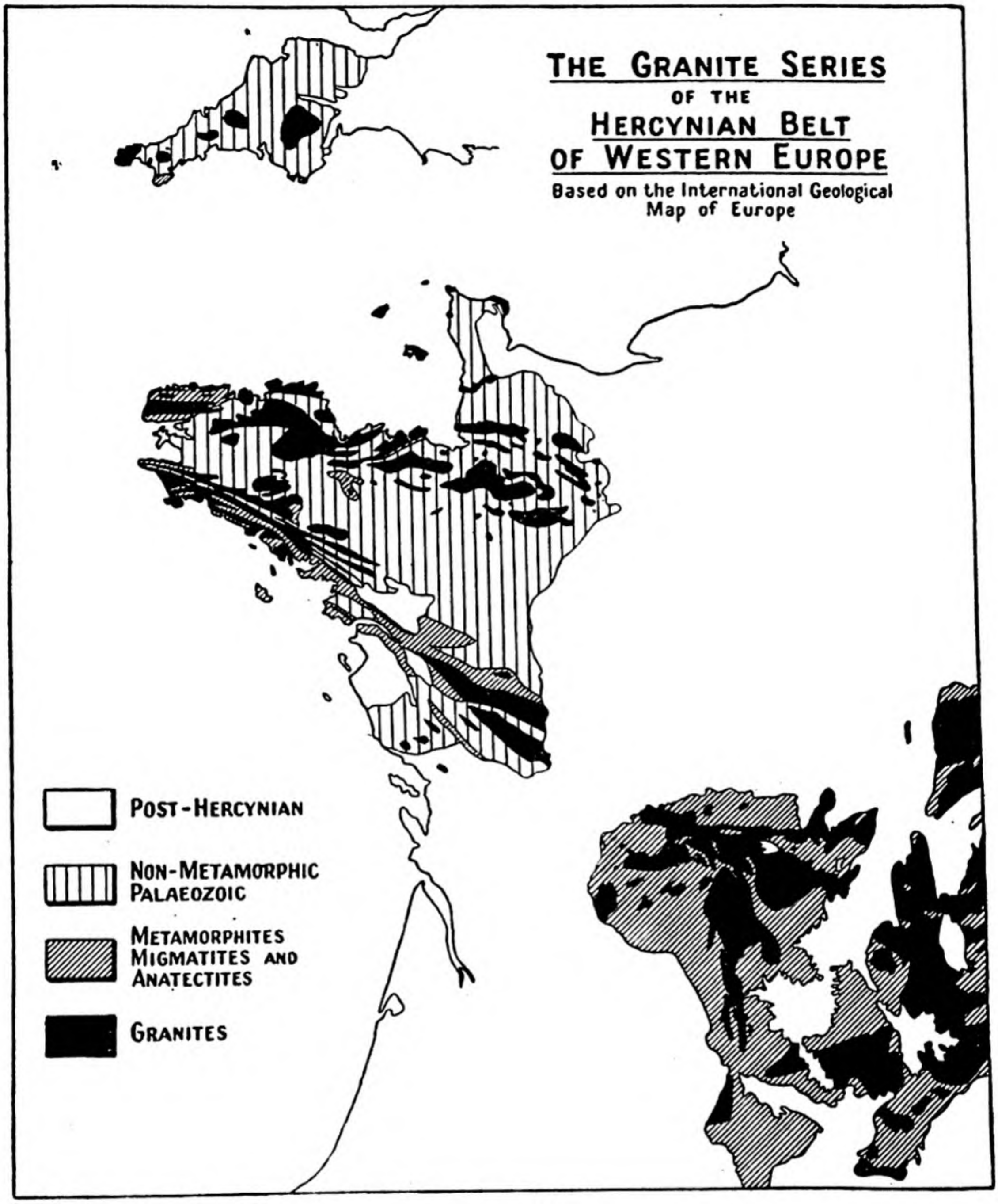


FIG. 18.

effectively in the crust only by the time the orogenic stress was diminishing'.

We can display the spreading-out in place and time of the Granite Series by a synopsis of the phenomena in the western part of the Hercynian belt comprised in the Massif Central and Brittany in France and Devon and Cornwall in England (Fig. 18).

The spread in place is at least 600 miles, in time the granites are alive till the Visean. As we have seen, in the Massif Central there are great developments of anatectic, deep or autochthonous granites associated with migmatitic and metamorphic rocks—the sedimentary rocks affected are Pre-Cambrian and Lower Palaeozoic. Following on these diffuse granites is a genetic series of more clearly intrusive types till the true intrusives penetrate the Visean. In Armorica, many of the innumerable granites are concordant with elliptical outlines, but some are discordant and cross-cutting; their emplacement has been guided by Dinantian axes and most of them are Carboniferous in age; the granites of the deeper structures have often gneissose and felspathized zones about them, those in the higher structures are more discordant, with hornfels aureoles. In South-West England, the Carboniferous granites were almost dead when they arrived in their present positions, they have no felspathized zones associated with them, they exhibit the *Granit-tektonik* of viscous masses. Viewing this portion of the Hercynian belt as a whole, we see the later members of the Granite Series appearing at higher levels as we pass from south to north in the direction of the tectonic transport. The higher and younger members of the north have left their deeper and earlier roots in the Massif Central. The simplifications of Fig. 18, which is based on the International Geological Map of Europe, yet reveal the fundamental differences of pattern at the three or four different sample levels of the Granite Series. There is no need here to develop the obvious relationships between place and time in the making of the patterns.

At the beginning of my last year's address, I announced that my prime intention in considering place and time in plutonism was to make my geological setting a little more comfortable. I confess finding a great deal of comfort, some of which may perhaps be the bliss of ignorance, when I view the Granite Series. I commend it to your attention for contemplation and comment.

A personal postscript.—During the last dozen years it has fallen to my lot to prepare half a dozen presidential addresses. I concede that on each of these occasions I have greatly enjoyed doing what I was called upon to do, especially as the time and place in which

I found myself provided only sedentary antidotes to the current distractions. But to every geologist, however decrepit, there must come a time when meditations, commentaries, contemplations and suchlike exercises begin to lose their savour. He feels the need for refreshment at those bounteous springs of field-work, and of the tonic given by the discipline of laboratory study, which have been denied him. Accordingly, I have taken such steps as are possible to ensure that the active years left to me shall be spent in the proper study of the plutonic rocks. In this intention, I have the good wishes at least of those who have suffered under these interminable and often pedagogic harangues.

VII

METAMORPHISM AND GRANITIZATION

1951

I. INTRODUCTION

I desire first to express to the South African scientific societies concerned my sincere appreciation of the great honour they have done me by inviting me to give this Du Toit Memorial Lecture, the second of the series but the first to be given by an overseas geologist. I accepted this invitation all the more readily because it gives me an opportunity of paying my tribute to the memory of one of the greatest geologists of our time. I need not attempt to assess the geological stature of Alexander Du Toit as it has been done from first-hand knowledge by Professor Gevers in the first Memorial Lecture delivered two years ago. My own hard-rock field, a field from which I have strayed but little, is only a small corner of that covered by Du Toit, but in this corner he made discoveries of significance not only for South African but for world geology. He introduced the all-important time-dimension into the study of the ancient rocks of this sub-continent and, under his hand, the dead rocks of the Basement came to life. He had began to write the history of the most fascinating and most obscure portion of the earth's crust.

In these investigations, Du Toit, like other workers elsewhere in the Basement, had come to recognize the importance of those processes of penetration, permeation and admixture which we group under the heading of granitization. For the last dozen years this same topic has been much in my mind and, accordingly, I have no hesitation in making it the basis of what I say tonight. I am fortified in this decision because the study of granitization

depends fundamentally on field-work. I consider it fitting that this examination of a branch of geology in which field-work is of the utmost consequence should be offered as a memorial to him who has been acclaimed 'the world's greatest field-geologist'.

Before proceeding to this examination, I wish to make a couple of references of a more personal character. I had the pleasurable and profitable experience of spending some weeks with Du Toit in the field in India and then came under the spell of that attractive but elusive personality that Professor Gevers has so admirably portrayed. Of that brief encounter I retain the happiest memories, geological and otherwise. Second, I find it gratifying, as a former student and now a professor at Imperial College, London, to recall that Du Toit obtained a portion of his geological education at that institution whence he moved on to Scotland, the scene of my major field-experience. From these recollections I derive the satisfaction that my association with him is not so slight as it might well have been; contacts even of these tenuous kinds have their spiritual values.

My final preliminary remark has something of an apology about it. It has been my duty in the last decade to deliver half a dozen presidential addresses and, in those, I have dealt at length with the metamorphic and granitic rocks. On this present occasion I do not propose to seek a new field. As a consequence of this timidity, some of what I have to say I have said already—but I trust this will be condoned. Presidential addresses can be long and dull, a lecture must be brief and may, perchance, be more lively. Further, I recall a statement of Du Toit himself: 'There is some danger lest the science become stereotyped through too close an adherence to accepted beliefs'; applying this completely personally, I can tonight continue my own geological evolution that is displayed in the addresses I have mentioned. This latest evolution, moreover, is influenced by the two months' experience that I have now enjoyed of South African geology and geologists. It is all too likely that anything I say concerning the ancient rocks of your country will be immediately vitiated by my ignorance of the subject; in that case, you can enjoy yourselves by showing how wide of the mark I have been. I requote the words of the great Hutton, the founder of geology:

'While man has to learn, mankind must have different opinions. It is the prerogative of man to form opinions; these indeed are often, commonly I may say, erroneous; but they are commonly corrected and it is thus that truth in general is made to appear.' (*The Theory of the Earth*, Vol. I, 1795, p. 250.)

The actual construction of this lecture has been delayed till this week—indeed, till this morning—so that I could utilize to the full my experience of South African geology. This circumstance has been in some measure a handicap, but I have been fortified for the past two months by the repetition of your excellent saying, *Alles sal reg kom**—I hope it proves to be true.

II. THE SUBJECT

I propose to examine the relation between metamorphism and granitization. Holding as I do that the commonest rocks are the most important rocks to the geologist, then I need make no apology for considering the metamorphic and granitic rocks—for they make the major part of the accessible crust. Many geologists here present may have decided, thus early in my remarks, that there is no relation between these rocks. Nevertheless, it has long been my thesis and will be again now, that metamorphism (properly defined), and granitization (also properly defined), are often genetically connected operations. But, to begin with, it is necessary to recall the senses in which I use these and some associated terms; the discussion of the validity of my definitions has been given in detail in my general papers listed in the Bibliography.

First, what do we mean by *granite*? Granite is a rock composed of quartz, alkali-felspars and a ferromagnesian mineral, in grains sufficiently large to be distinguished by the naked eye, and possessing a texture produced by the crystals as a whole interfering with one another's free development. For our purpose, granites include the granodiorites. We can base this definition on a loose piece of granitic rock. If we use the word igneous in the definition we are bound to show that the rock in question has consolidated from a magma, molten rock-material; and nobody has seen a

* *Afrikaans:* It will all come right.

granite magma consolidating into granite. The origin of each and every granite can therefore legitimately be discussed.

Next, we have *granitization*. This means the process by which solid rocks are converted to rocks of granitic character without passing through a magmatic, or liquid, state.

Metamorphism comprises the processes which, working below the shell of weathering, cause the crystallization, recrystallization, or reconstitution of the original materials in rocks, no general simultaneous melting or solution of the rock taking place. *Regional metamorphism* affects rocks regionally, over a great expanse of the crust; that is the proper non-genetic definition.

I shall use repeatedly the terms *migmatization* and *migmatite*. The migmatites are mixed rocks, resulting from the mixture of any older rock with introduced material. This introduced material is often said to be magmatic, but that has to be proved in every case. Indeed, the nature, amount, and function of the introduced material provide the major problems of the migmatites. This material may vary, it seems to me, from magma to the most tenuous fluids capable of metasomatic action. The product may vary from a homogeneous rock produced by soaking to a coarsely mixed rock easily separable into relic and granitic or granitized portions.

There is, as I show immediately, a very close association in the field of metamorphic, migmatitic and granitic rocks. I believe this association to be one of great genetic significance—though it is true that some geologists consider the association to be fortuitous and the whole problem of metamorphism to be confused by it. Because I feel so strongly on this point I have proposed to unite these three groups into one fundamental unit which I call the *Plutonic Series*, the true rocks of the depths. What I have to say concerns the Plutonic Series.

III. THE PLUTONIC SERIES

Whether the Plutonic Series exists or not can be proved only in the field. I could cite from all parts of the globe examples of the field-association of the three terms, metamorphites, migmatites, granites. I have time now only to present a few cases—I select

one from each of four continents to indicate the ubiquity of the association.

First, let us consider my friend Peter Misch's work in the high Himalayas, in well-exposed ground, deeply dissected. Misch (1949) traces a series of zones of increasing metamorphism, beginning with slate, passing through phyllites, mica-schists, garnet-mica-schists, kyanite-gneisses into sillimanite-gneisses and ending in a zone abounding with granite—a zone of granitization. The metamorphic zones stand vertical and cannot be due to depth-control or dynamic action. They must be genetically related to the heat and material supplied by the granite emplacement, whether this is a magmatic or migmatitic operation. Misch (1949, p. 245) concludes as follows:

'Finally, I wish to emphasize the general validity of the statement that high-grade regional metamorphism and synorogenic granitization are invariably and necessarily linked. They are expressions of one and the same process. There is not any region known to me either from study of the literature or from personal field-work, where katazonal or lower mesozonal crystalline schists occur without metamorphic migmatites or granitic rocks.'

Second, we may take an example from Scotland, an especially good example because it involves the classic series of metamorphic zones established by the pioneer work of George Barrow some sixty years ago. In the South East Highlands, Barrow delineated an orderly succession of metamorphic zones in pelitic rocks, each zone being marked by the incoming of a significant index mineral. The lowest grade is characterized by chlorite and successively higher grades by the appearance of biotite, almandine, staurolite, kyanite and finally of sillimanite. These highest-grade sillimanite-gneisses are intimately associated with diffuse granite material, the 'older granite' of Barrow. I showed in 1927 that the sillimanite zone was essentially a region of granitization. Whether this interpretation is accepted or not, it is reasonable to regard these classic metamorphic zones as something in the nature of a gigantic thermal aureole about a focus of granitic activity.

For a third example we turn to the United States, and select that from Dutchess County, New York State, described by Balk and Barth (1936). Here the Hudson River Slate shows three facies

from north-west to south-east, giving a succession of muscovite-slate, kyanite-schist and sillimanite-gneiss—a succession not related to any depth-control but, as Barth says, determined by an increase of granitic material in the higher-grade facies. The transitional series is regarded as the result of a regional metasomatism and permeation brought about by solutions derived from magmatic rocks occurring to the east. Barth (1936, p. 830) states:

'The fact that the function exercised by the solutions in this area is that of changing an argillite into a granite seems decisive proof that the ultimate source of the solutions must be sought in the intrusive rocks.'

Lastly, I present a South African case. I had the good fortune to visit the Tugela Valley in Zululand, the scene of du Toit's pioneer work, under the guidance of Professor Gevers of the University of the Witwatersrand. North of the Tugela about Jamieson's Drift, the Mfongosi Series shows an increase in metamorphic grade from low chlorite-schists through coarser hornblende-schists into veined amphibolites; south of the river, these amphibolites pass into dioritic gneisses with relics of less migmatized country-rock. The metamorphic grade increases towards a migmatite area.

As I have said, there are innumerable similar cases. When we follow rocks into higher metamorphic grades we finally end in a granitic core. Whatever the origin of the granitic rocks of this core may be, whether consolidation from magma or transformation in place, the field-evidence holds good. This cannot be accidental, the association of metamorphites, migmatites and granites must mean something. Whether we shall be able to agree on what it does mean remains to be seen, but I hope we can all agree that there is a Plutonic Series.

IV. GRANITIZATION

In what I have said so far, I have implied that opinion is divided as to whether the granitic rocks that make the core of the Plutonic Series are igneous rocks formed by the consolidation of magma or metasomatic rocks formed by the transformation of the country-

rocks into granite in place as they stood. The debate lies between the magmatic pontiffs on the one side and the migmatitic soaks on the other—pontiffs versus soaks. This nomenclature of the disputants is not one invented by me for this entertainment—it is already enshrined in the literature of the discussion and indicates the excellent, desirable and enjoyable manner in which the debate is being conducted. Before I go farther, however, I have to make a plea that no one should become dogmatic or authoritarian on this matter; no friendships, doubly precious at this time, can be cast aside. We must judge every sample put before us by the field-evidence, reinforced but not dominated by petrographical, chemical or any other testimony we can bring forward.

I have therefore now to consider the validity of granitization which, you will remember, is the process by which solid rocks are converted into granite without passing through a magmatic stage. I have dealt elsewhere at some length with the history of the development of ideas on this proposition. It is sufficient on this occasion to recall that, as is so often the case in geology, these ostensibly new ideas were propounded at the beginning of our science. A century ago, for instance, French geologists held that country-rocks could be made over into granite, and some of the terms they then used have been reinvented today. We may pause to look into what may be the cause of the resurrection of these ancient granitization proposals. I think it may well be due to a reaction on the part of field-geologists against the dominance in hard-rock geology of what passes as physical chemistry. A geologist must be a geologist first and foremost—other disciplines must be servants and not masters. This is especially true, I feel, in the study of metamorphism and granitization where field-evidence is the chief and decisive evidence. If granitization, for example, cannot be proved in the field, then it cannot be proved at all—it is as simple as that. Many of us, fortunately, prefer the field to the crucibles and the triangles and more complex geometry that seems to go with them. (For my own part, I have to confess that I never did understand what many of the triangles meant; I have, in fact, a great deal of sympathy with the Victorian statesman in the matter of those 'damned dots'). Let us go to the field, therefore, for evidence of granitization.

The first body of this evidence is provided by what I call *ghost*

stratigraphy and *ghost structure*. In brief, this means that the stratigraphy and structure of the adjacent country-rocks can be traced through a granite as if the granite were not there. The great French masters of granitization were familiar with this phenomenon more than half a century ago. Barrois traced bands of quartzite through granite, the associated argillaceous rocks having been granitized; Michel-Lévy followed pelite bands through granite by the presence of cordierite knots; Lacroix showed that limestones were recorded in granite by hornblendic and pyroxenic types. In modern times, Misch has recorded the occurrence in the Nanga Parbat region of 'pure white marble forming regular bands of small thickness but great longitudinal extent immediately in the coarse-grained gneissose granite'. He presents a figure showing such a thin marble band, standing nearly vertical, and visibly maintaining its course and attitude undisturbed in gneissose granite perfectly exposed in a region of great topographic relief (Misch, 1949, p. 215 and Text Fig. 2).

The examples cited above might be interpreted by the obdurate as being screens or relics of screens separating two magmatic intrusions. The example I now give cannot be interpreted in this way—it comes from the ground in Donegal, Ireland, now being investigated by my research school and it provides the standard pattern of this class of evidence. The ground was mapped by Dr. A. R. Gindy and Dr. W. S. Pitcher—detailed accounts of their work have been presented for publication to the Geological Society of London*. The country-rocks consist of a rapidly alternating series of metamorphosed siliceous, semipelitic, pelitic and carbonate rocks, with thin and thick metadolerite sills—the whole being folded and lineated before granitization. The stratigraphical succession can be determined with confidence from graded and current bedding. Thousands of enclaves, large and small, occur within a granodiorite and maintain the same lithological succession and the same detailed structural pattern as the parent country-rocks outside the granodiorite. The lineations of the country-rock and of the enclaves within the granodiorite have the same attitude. Observations of lineation deal, after all, with the attitude of a line—it is obvious that during the emplacement of the granodiorite there has been no disturbance of the smallest piece of

* Now published: see Gindy (1953) and Pitcher (1953*a*).

country-rock now enclosed in it. Stoping or magmatic assimilation are clearly inadequate to explain such ghost stratigraphy and structure—the only explanation appears to be that the country-rocks have been transformed into granodiorite on the spot, they have been granitized.

Most of the enclaves in the Donegal migmatites are of quartzite, limestone or metadolerite; it will be recalled that similar rocks make the enclaves in certain of the French examples and in Misch's Nanga Parbat case. A moment's reflection will show that such was to be expected. These rocks are of extreme chemical types which require a great expenditure of energy and a great inflow and outflow of materials to convert them into granite. They contrast violently with the semipelitic and pelitic rocks which easily succumb because they are not so widely different from granitic rocks in bulk composition. It is obvious that rocks will not react equally to granitization—those, such as quartzite, limestone, metadolerite and serpentine, that prove stubborn are what I call *resisters*. I suggest later that such resisters are of considerable significance in the development and control of regional metamorphism.

Because of this differential reaction to granitization shown by rocks of varying composition and texture, there becomes available a method of testing what actually occurs during the transformation of any layer. A narrow band of susceptible sediment flanked on either side by more resistant beds may be considered to provide an elongated chamber in which the progress of granitization can be observed. This method has been used by my former student Dr. Y. C. Cheng in the migmatitic area of Sutherland, Scotland. Cheng (1944) has investigated the textural and mineralogical changes that are revealed in such favourable thin bands as they are followed along strike towards the granitization core. The passage of such sealed-off bands of mica-schists into permeation-gneisses and granitic gneisses provides unassailable evidence for granitization. I may here mention another aspect of Cheng's work; study of the textures of migmatites and granitization granites shows that the final picture as we now see it is the result of a lengthy and complex series of metasomatic replacements; Cheng is able to separate nine main stages in this process which are further divisible into more than a score of distinct episodes. The textures

of such rocks are by no means those arising by magmatic crystallizations.

On occasion we may be fortunate enough to observe the preservation of significant sedimentary patterns and structures in granitization granites. The most spectacular example is provided by granitized tillites. Unequivocal cases of these come from the pre-Cambrian tillites of South Australia and New South Wales. Dr. Donald Bowes, another former student of mine, has demonstrated the gradual transformation of the arkosic and greywacke matrix of a South Australian tillite into coarse granite with the preservation of old granite and quartzite erratics in their original positions—incidentally providing another example of differential resistance to granitization*. (Some apology may be necessary for this constant citation of the work of my own school—but we really do know more than most about the field facts of granitization.) Mr. Hadden King, Chief Geologist of Zinc Corporation, Broken Hill, N.S.W., has recently shown me magnificent photographs illustrating the same transformation from the New South Wales area; here the final stage of the process, the making-over into granite of the hitherto resistant erratics, is excellently displayed. By Mr. King's kindness, I am able to project a series of lantern-slides demonstrating the whole operation; it is to be hoped that his complete evidence will soon be published.

I think that all of us would admit transitions, on some scale or other, from country-rock to granite. The magmatists would hold that migmatitic rocks are simply marginal mixtures of granite magma with country-rock; migmatists would hold that the granite is simply the final stage of the process begun in migmatization. In some exposures, however, it may be possible to make a choice between the two interpretations. It is often found that identical and peculiar felspars occur in granite and also in the adjacent migmatite, here embedded in undoubted country-rock. Unless we assume that complex felspars can form in two entirely different environments, one that of magmatic crystallization and the other that of soaked but still solid country-rock, then we can only infer that the felspars of both the granite and the country-rock have had one origin, by growth in the solid. It is felspathization of sediments that can produce granitic rocks.

* Now published: see Bowes (1954).

Beautiful examples of such granitization have been shown me in the Umgeni Valley in Natal by Dr. Frankel of Durban and on the Natal South Coast by Professor Gevers, and I saw square miles of such rocks in Namaqualand under the guidance of Dr. Nel and Dr. Truter of the Union Geological Survey. These examples are of a very great size, but similar phenomena are seen down to pocket-size in all granites—later I cite an excellent case from one of the Cape granite bodies. All reasonable geologists, I feel, would accept this field-evidence for granitization; they part company when they begin to apply, or not, these results to the whole substance of granite bodies. *It is a fundamental thesis of my remarks tonight that the scale of any act of granitization can be judged only from the regional setting of the granite body at the crustal level at which we now see it.* Large-scale granitization, applicable throughout a granite mass, must be accompanied by the appropriate apparatus of migmatites and metamorphites. A small and local demonstration of granitization in the wrong regional setting means nothing so far as the origin at that level of the main body of the associated granite is concerned. The profound importance of the regional setting in the interpretation of granites has been brought home to me by my experience of your South African occurrences. I consider it to be of so great a significance in the general theory of granitization that I feel compelled to return to it again and again. We may start its discussion by considering my reaction to the most famous granite contact in the world, that at Sea Point, Cape Town.

My thanks are due to Professor Walker of the University of Cape Town for guiding me over the Sea Point and other contacts of the Cape granite. I have to confess that I was first shocked and then relieved by what I saw at Sea Point. In the past, I have used the development of identical felspars in the Malmesbury rocks at the contact and in the adjacent granite as indicating granitization. The exposures reveal that nothing of the kind has occurred there. The contact is a moved one, showing clearly the mechanical mixing of softened-up Malmesbury slate and viscous granite material (Scholtz [1946, Plate iv] illustrates this contact). It is true it is a mixed contact, but of a mechanical kind. I have just said that after being shocked I was relieved. My relief arose from the realization that the Cape granite body was in the wrong geological setting for large-scale granitization; the country-rocks were low-grade

phyllites, scarcely metamorphosed, that had been thermally altered near the granite contact—there was no development of two of the three members of the Plutonic Series, the migmatites and metamorphites. The Cape granite body was, in fact, a pluton in the classification of granite masses which I call the Granite Series. I remind you of this classification later on—it is sufficient now to state that the plutons are emplaced nearly solid and are not associated with granitization at the level at which they are now found.

After having decided that the Cape granite was a pluton in my classification, I had the good fortune to be conducted over many such bodies in the Cape Province by Professor Scholtz of Stellenbosch, who had called them plutons in his own classification (Scholtz, 1946). I wish to thank Professor Scholtz for his great kindness in his matter. The contacts that I saw were again of the moved plastic type. At Swellengift on the Diep River, there is again a mixture of softened Malmesbury sediments and viscous granitic material. An instructive roadside section is available on the Hopefield road from Malmesbury at the northern end of the Paardeberg pluton; at the contact the Malmesbury rocks have clearly been softened up and made plastic—they are now violently contorted gneissose rocks contrasting sharply with the steady well-behaved phyllites away from the contact. The Darling pluton likewise shows a streaky mixed contact as seen near the Darling-Mamre road. I suggest that the Cape plutons, like the other granite bodies that I call plutons, made their way in as nearly solid masses by softening-up the country-rocks round about them. Their setting is correct for this interpretation. As I have said elsewhere, we can usually tell more about the manner of emplacement of a granite by study of the adjacent country-rocks than of the granite itself.

I mentioned a moment ago that even in the wrong setting pocket examples of granitization could be observed. Thus, Professor Scholtz showed me, on the coast north of Groot Paternoster near Saldanha Bay, great rafts of metasomatized Malmesbury sediments felspathized and soaked and veined by granitic materials; this zone passed fairly rapidly into porphyritic granite of Cape Type, containing rounded relics of transformed sediments, which gave place to homogeneous granite. From such a section much

detail of granitization can be observed and many valid conclusions may be drawn—but the conclusion that all the granite of this pluton is of *granitization origin at its present level* is certainly not valid—once again I have to remark that the regional setting is against it.

From the considerations that I have now put before you, I draw the general conclusion that granitization must be on the largest scale to provide the granites that form the cores of the Plutonic Series of granites, migmatites and metamorphites. Other granite bodies, not immediate members of the series, may supply evidence of small-scale granitization but it would be unwise to apply this interpretation to the whole substance of such bodies.

You will expect me to say something on the mechanism of granitization and migmatization, seeing this topic is one of lively debate in Europe and America. To many, a magma is necessary from which come fluids, ichors, juices and so forth—such materials emanate, and we may loosely group them as emanations. To others, no magma is required, the emanations existing in their own right. Among the transformists we can separate two schools of thought, both believing in diffusion of material in granitization; there are those, the *Drys*, who put their faith in ionic migrations, with balanced additions and subtractions, involving the production of concentrations of various kinds, and those, the *Wets*, who hold to liquid diffusion. In this matter, I am definitely with the Wets. I have no time to establish my position in full detail, but it seems to me that there are innumerable evidences in most granitization regions of the operation of fluids in permeation, soaking and all the processes of migmatization. The work of solutions is indicated by the shapes of the boundaries between grains, by corrosion-surfaces and warty junctions and by the formation of hydroxyl minerals. A means of transport of quantities of material and energy is required; import, reaction and export have to be promoted. It seems to me that all this is better done by a fluid phase than by diffusion in the solid. Further, the Drys, by an excess of logic, have applied their theory to explain phenomena which normal and classic geological reasoning has explained perfectly (and differently) already. To be blunt, some of the Dry dogma appears so preposterous that I begin to suspect the validity of the whole of it—an illogical but natural reaction.

We may now consider for a moment the question of fronts, remembering as we do so Bowen's aphorism on the matter—what is a front to one man, is an affront to another. The movement of material and energy into a part of the crust may result in the expulsion of other material into the surroundings—a wave-like movement leading to the setting-up of certain diffusion limits marked by changes in composition, the limits being the fronts. The concept of fronts is a useful and fruitful one, and I use it in a moment in the consideration of the main topic of this lecture. Before doing this, however, I must mention the most famous front of all, the Basic Front favoured especially by Dr. Doris Reynolds of the Edinburgh School. She maintains that the influx of NaCaSi into a locus of granitization results in a complementary expulsion of AlFeMgCa into the surrounding rocks, where is built up a zone enriched in these materials and giving biotite-rich hornfelses and even basic and ultrabasic bodies—the Basic Front.

I have to confess that there are certain aspects of the proposition of the Basic Front that have come to cause me some disquiet. Basic Fronts cannot be a necessary and requisite accompaniment of granitization—many great migmatite-complexes appear to be devoid of them and many granitization-granites are richer in FeMg than the country-rocks from which they were formed. What causes me most uneasiness here, however, arises from my insistence on the regional setting being right for granitization. The examples of Basic Fronts as demonstrating granitization advanced by Dr. Reynolds are mostly in regions of non-metamorphic or lowly metamorphosed country-rocks; the granites themselves are what I should class as plutons, the high-level and late members of my Granite Series. Granitization in such a setting can be, I hold, only a minor and small-scale affair incapable of producing the desired wholesale transformation. The origin of the Basic Fronts may still then be a problem. I have in another place (Read, 1948*b*, p. 195; this volume, p. 254) commented upon our good fortune in having homogeneous sedimentary rocks as the country-rocks involved in the making of the most celebrated Basic Front, that of the Newry granodiorite in Ireland, and that therefore there can be no doubt about the nature of the transfer operating. This statement now needs reinterpretation; the country-rocks are homogeneous enough to permit estimates to be made of changes in

their composition at the granodiorite contact—the mode of production of these changes has now become for me a rather open question.

It may be that we have got hold of the wrong end of the right stick. I suggest for discussion that some basic fronts may be better interpreted as *Basic Behinds*—a somewhat indelicate term, I admit, but one which expresses the possibility that we may here be dealing with subtraction-rocks, with rocks from which for example NaSi has been extracted with the consequent piling-up of AlFeMg. The biotite-rich streaks and bands which are characteristic in the granitic portions of migmatitic granites—beautiful examples were shown me by Dr. Truter in the Namaqualand migmatites north of Dabenoris near the Orange River—are reasonably interpreted as basic behinds; the quartzo-felspathic ingredients of the original country-rocks have become incorporated in the granitic layers, leaving the residual country-rock relatively enriched in ferromagnesian components.

Mention of that truly delightful country, Namaqualand, recalls to me one of the many problems of the migmatites that may possibly be solved by the subtraction proposal; it is that of the occurrence of great bodies made of aluminium silicates and sometimes alumina within migmatitic complexes. In India I have seen hills of pure kyanite in the midst of granitic and migmatitic gneisses; a Namaqualand example is that of Annakoppie near my beloved Pofadder, a great body of sillimanite-corundum rock in migmatites. The current explanation of such bodies as metamorphosed bauxites seems to me to raise formidable problems of palaeogeography and stratigraphy that have not been adequately, if at all, regarded. I suggest that these occurrences might be due to subtractions connected with metamorphic differentiation operating on a grand scale in the juicy setting of migmatization.

Now that I am fairly launched on a sea of suggestions concerning the Namaqualand migmatites, I can comfortably continue in that country. I heard much, but saw nothing, of a mysterious and remarkable monazite deposit near Van Rhynsdorp that was encased in migmatites; those concerned in the investigation of this deposit might contemplate the suggestion, even though they may cast it aside, that they are dealing with a body that is at the same time a subtraction-rock and a resister. The same suggestion can be made

for the Namaqualand copper deposits north of Springbok. I am indebted to Dr. Wiid and his geological staff of the O'Okiep Copper Company for instruction in the geology of their field and for many other kindnesses. (When meat becomes a rarity in England, I shall feast on the memory of Dr. Wiid's sumptuous braaivleis.) Before I left Springbok, I was invited to interpret the Namaqualand copper deposits—I accepted the invitation most willingly, explaining that my knowledge of economic geology was nil, but obtaining confidence from the circumstance that the next day I would be hundreds of miles away from Springbok, and in a few weeks in another continent. I suggest that the so-called norites with which the copper is associated are not intrusives later than the regional granitization. Their form and distribution—there are some hundreds of exceedingly irregular bodies occurring in a broad but distinct stratigraphical zone in the migmatites—seem to me to show that they are resisters derived from original copper-bearing basic flows, sills or pyroclastics that formed part of the country-rock before granitization. During this process, their original layers became dismembered and, as the process advanced, there was a concentration of ferromagnesian and copper-bearing minerals within the fragments. These are thus resistant subtraction-rocks. The ore-bearing bodies appear to me to have undergone some migmatization and to fit into the general migmatization pattern. They occur on small steep structures in the flatly foliated migmatites—a circumstance due, I suggest, to their being tougher pips in the slightly plastic gneisses. There seems to me to be a connexion between the degree of mobilization of the gneisses, the shape of the ore-bearing bodies and the ore-content. In the more mobilized parts of the gneisses, as at the old Tweefontein Mine, the copper-rich ore-body has a stream-lined platter-like shape consistent with the trimming of an irregular body during plastic flow of its surroundings; as mobilization advances, subtraction advances, so that the stream-lined bodies are richest in ore and in 'noritic' constituents. I fly these kites—perhaps a few will still be flying when the experts have finished shooting.

V. THE ORIGIN OF REGIONAL METAMORPHISM

I have already cited sample cases of the close association in the

field of the regionally metamorphosed rocks with granitization products. This association can be interpreted in the light of what I have just said concerning the granitization process. Out from the central theatre of granitization there pass waves of metasomatizing solutions, changing in composition and in temperature as they become more distant from the core and promoting thereby the formation of zones of metamorphism about it—I here use words that I wrote a dozen years ago. Regional metamorphism is thus a permeation of country-rock by metasomatizing solutions. The validity of this proposition is not lessened if we believe, as the magmatists do, that granitization does not occur and that migmatization is a local phenomenon attached to the intrusion of a granite magma.

There are innumerable features of the regionally metamorphic rocks that support a metasomatic origin. The preservation of delicate original textures, such as varves, current bedding and others, and the coincidence of schistosity and bedding appear to be reasonably explained along those lines.

If this suggestion is correct, then there might be expected a change in composition of the original rocks on metamorphism, a change increasing in intensity as the granitization core is approached. An indication that this may indeed be the case is provided in the pioneer work of Lapadu-Hargues (1945) who has collated the chemical analyses of a series of types constituted by slates, sericite-schists, biotite-muscovite-schists, muscovite-biotite-gneisses, biotitic granitoid augen-gneisses, granites and muscovite-granites. If these seven types are regarded as a progressive series, Lapadu-Hargues finds that with increasing grade (1) the alkalis : alumina ratio approaches unity, that is the felspar ratio, indicating an influx of alkalis from deeper zones, (2) in the low grades, there is an accession of soda, in higher grades the accession of alkalis continues with potash now dominant—both alkalis come from a deep source, the soda moving the farther, (3) iron oxide and magnesia decrease with increasing grade of the rock, becoming concentrated in the lower grades, and (4) lime shows a progressive increase with grade. Lapadu-Hargues thus proposes a progressive calc-alkali influx combined with an iron-magnesia migration into the lower-grade zones—giving the appearance of a kind of basic front.

This work of Lapadu-Hargues is only a beginning—but a beginning full of promise. We need, I have suggested, a statistical study of the variation in chemical composition of the metamorphic, migmatitic and granitic rocks keeping the big atom, oxygen, constant. This is a task for the future; we can decide for the present that there is likely to be demonstrated a change in bulk-composition of the Plutonic Series of rocks that is consistent with their genetic unity. Lapadu-Hargues's results agree with the nature of the transfer of materials shown in those granitization complexes that have been chemically investigated. It is possible, as I have proposed on several occasions, that we see an indication of this general movement of material in metamorphism in the ubiquitous occurrence of tourmaline in rocks of all grades—the boron atom may reach the outermost bounds of the metamorphism; I must confess, however, that some geochemists of authority do not agree with me in this interpretation.

If it is admitted that regional metamorphism is associated with the passage of energetic fluids through the rocks, then regroupment of the constituent materials is to be expected. This we see in the operations of metamorphic diffusion and differentiation—operations that become intensified in the juicier environments of migmatization. Individual minerals grow to great sizes, as is exemplified by the large garnets of the Namaqualand, Natal and other migmatites. I have already suggested that the alumina-rich bodies occurring in many migmatitic complexes are the result of these regroupment processes. We have to agree that the composition of any small unit of rock cannot be expected to remain constant during the plutonic act.

I have mentioned zones of metamorphism. It will become clear as my argument advances that any standard set of metamorphic zones or facies is unlikely to be produced by the processes I propose. This is unfortunate for the textbooks—but there it is. The variables are too many and diverse. The compositions, structure, texture and grain-size of the rocks undergoing metamorphism are clearly of great significance. The pressures and temperatures operative during the process can vary within wide limits. Moreover the waves of metasomatizing solutions moving through the terrain may be of diverse qualities and compositions; they may be wet or dry, and they may encounter wet or dry country-rocks. We may

have charnockitic migmatization in a dry environment and amphibolitic in a wet.

But among the most important variables in metamorphism is one that hitherto has been scarcely considered—it is that arising from the different reactions of different rocks to granitization. As I have already said, rocks such as quartzite, limestone and basic and ultrabasic igneous rocks form the resisters that require great expenditure of material and energy to convert them into granite. There may thus be provided structural barriers that shield off great masses of rocks from the granitizing and metamorphosing fluids. Metamorphic zones and facies may be different on the two sides of a resistant band. A couple of examples will make this matter clear—perhaps it would be better to say, an example and a possibility. Work, unpublished as yet,* of Dr. A. R. Gindy in Donegal shows that the core of a fold has been protected from granitization and metasomatic influences in general by a quartzite member of the sedimentary succession; the rocks inside the fold are of quite a different metamorphic facies from those outside in the migmatization area; further, the quartzite barrier appears to have dammed back the granitization fluids with the production of intense granitization along its contacts with the migmatites. The possible parallel case I have mentioned derives from my visit to the Barberton area of the Transvaal under the guidance of Dr. Willemse and Dr. van Eeden; I desire to thank these two geologists for their kindness on this and many other occasions—I am especially grateful for much invigorating discussion. It seems to me worth suggesting that the low-grade sediments of the Fig Tree and Moodies Series that form the core of the great Swaziland basin have been protected from metamorphism and migmatization by the barrier of resistant basic and ultrabasic rocks of the Jamestown Series. Beyond this barrier is the vast expanse of Old Granite, the Nelspruit Granite, which I interpret as produced by the migmatization of semipelitic and more siliceous rocks; this old granitic gneiss contains abundant relics of basic resisters, and a few of limestone, but its characters are such that it can result only from the replacement of more susceptible rock-types. In places the Jamestown resister-barrier failed, and the migmatization swept through to produce locally sillimanite-gneisses from the

* Now published: see Gindy (1953).

normally low-grade Moodies sediments of the basin; near such high-grade patches some of the Nelspruit migmatite clearly becomes mobilized and intrudes itself into the non-migmatized country-rocks.

This application of the resister-barrier idea to the variation of the metamorphic and other plutonic rocks of the Barberton and Swaziland areas might, I suggest, be considered by those familiar with the ground. Whether the idea is a fruitful one or not for this particular area, it will be conceded that variety in metamorphic grade is to be expected to arise by the association of resisters and non-resisters in the country-rocks affected by plutonic processes.

In addition to the many and important variables and controls in regional metamorphism that I have so far mentioned there yet remains what may well be the most fundamental variable of all, and this is time—the time of the operation with respect to the general history of that part of the crust. I develop this aspect of my theme later on—but I hope it is now evident that the possibilities in regional metamorphism are so numerous that all metamorphic zones and facies are restricted in application and meaning. There can be no 'standard' set of zones. Whilst each metamorphic terrain conforms to a general pattern, the diversity in detail is infinite. It is therefore necessary, at least at the start of the study, to consider each metamorphic region as *sui generis*—two words that might well be adopted as a motto by plutonic geologists. It is instructive to recite some of the synonyms of *sui generis*—I select the following from the many in Roget's *Thesaurus*: exceptional, abnormal, anomalous, irregular, aberrant, peculiar, eccentric, unusual, uncommon, singular, unique *and even* lawless, stray, wanton, egregious, curious, odd, monstrous, queer, quaint, fantastic and funny. I feel that the Plutonic Rocks in their infinite variety illustrate every one of these renderings.

The general proposition that regional metamorphism and granitization are interconnected processes will doubtless not be accepted by all. There are those who equate regional metamorphism with dynamic metamorphism, orogenic pressures being the operative factor. It is supposed that metamorphism is induced in rocks because of their deformation. That this is unlikely is indicated, I believe, in the common coincidence of schistosity and bedding and by the preservation in completely recrystallized

rocks of delicate original structures such as current and graded bedding, varve-structures and similar fine arrangements. Because of these and other difficulties in the dynamic interpretation, there has developed the proposal of load metamorphism induced by the vertical pressure of overlying beds. Load metamorphism is impossible on a variety of counts: immensely thick piles of sedimentary rocks, 50,000 ft. and much more, are completely non-metamorphic; high-grade metamorphic rocks overlie lower-grade; low-grade rocks pass into higher-grade rocks in very short distances. Neither dynamic metamorphism nor load metamorphism appears to meet the observed field-developments of regionally metamorphosed rocks. They are not by themselves enough—I suggest there is needed in addition the metasomatizing activities set in motion by granitization.

Of course, the grade of metamorphism often increases with depth, but it does not increase because of depth. The domain of metamorphism usually becomes nearer the source of granitizing solutions with increase of crustal depth, but these solutions may travel laterally and even, I suppose, downwards. If we interpret metamorphic zones as being in some measure a stratigraphical succession, then I feel we may easily be led into deductions concerning the tectonic and metamorphic history of a region that may be erroneous. Metamorphic inversions are, as I have explained in another place, particularly dangerous concepts.

Anyone who advances a general principle does well to search for the exceptions. Now that I have put before you the thesis that regional metamorphism and granitization are genetically connected, I must consider whether or not this is of universal application. Are there widely extensive developments of metamorphic rocks unconnected with granitization? This question we may proceed to examine.

VI. APPARENT EXCEPTIONS TO A GENERAL RULE

In many regions of granitization and migmatization it has been shown that such processes have operated on rocks that had already been metamorphosed, though usually to a low degree. A few examples will suffice. Goldschmidt (1921) in the Stavanger area

of Norway distinguished a regional metamorphism of phyllite grade upon which had been superposed an 'injection-metamorphism' or migmatization that produced higher-grade schists with biotite and garnet and culminated in augen-gneisses. Misch (1949, p. 242) states that

> '. . . in the Nanga Parbat area, regional metamorphism without granitization did not go beyond the phyllitic (and perhaps uppermost mesozonal) stage. All zones of higher-grade metamorphism are in their spatial distribution so clearly related to the central rise of the migmatite front, that a genetic control is obvious.'

Misch (1949, p. 244) goes on to show

> '. . . that the statement that orogenic metamorphism without granitization is apt to achieve only low-grade mineral facies ("pure dynamometamorphism") is borne out by numerous migmatite-free regions of phyllites and basal-epizonal and upper-mesozonal mica-schists, and of other low-grade schists (greenschists, &c.).'

He instances one such example in the geosyncline of West Yunnan where there are many thousands of square miles of low-grade schists produced by dynamic metamorphism during Mesozoic orogeny—no migmatites of Mesozoic age are found anywhere in this great region. (See also Misch, 1949, pp. 373–4.)

The clearest separation between a widespread metamorphism and migmatization is that proposed by the French workers, Jung and Roques (1938; and Roques, 1941). The latter separates out two great sets of zones in the Central Plateau of France; the first, the *ectinites*, are formed with no accession of felspathic materials, the second, the *migmatites*, result from an alkali influx. The migmatite zones reach different levels in the ectinite zones and the two sets of zones are formed as rather distinct and separate events. Certain of Roques's conclusions, especially those concerning the degree of separation, appear to me to be open to criticism, as I have suggested elsewhere, but his general proposition is of course worthy of serious consideration. Two views at least concerning it are possible: the ectinite zones are the higher-level expression of an earlier and deeper-seated migmatization or else the ectinites or some portion of them are real, and provide

an exception to the proposed general association of regional metamorphism and granitization.

It is a matter of observation that there are very considerable areas of the crust that are violently folded and exhibit only a low grade of metamorphism throughout their extent. This metamorphism is associated with orogenic folding, and granitization is not immediately nor directly concerned with it. With Misch, we may call it *orogenic metamorphism*. Examples are familiar to all geologists and are widespread—the products are the slates and phyllites seen in large parts of any fold belt. In the Union, the Malmesbury slates in which are set the Cape granite plutons provide one case and the low-grade contents of the Swaziland basin another. Even in the abnormal fold-belt of the Alps, the metamorphism produced during the violent orogeny was dominantly of epizonal character and gave rise to phyllitic rocks; where the Alpine metamorphism reaches a higher grade the situation is one of concentrated tectonic action. The highest-grade metamorphism in the Alps is in the root zone where plutonic activity (in my sense) was active even during late Tertiary times. (For a general summary of Alpine metamorphism, see P. Niggli, 1950.)

This is possibly the best place in these remarks to interpose a brief consideration of a class of metamorphic rocks of some difficulty—those containing chloritoid. So far as my experience in the field of chloritoid-bearing rocks goes (and it goes some little way), I tentatively advance the opinion that regional chloritoid marks zones of specially concentrated movement. In Unst in the Shetland Islands of Scotland, I find such rocks truncating granitization metamorphites and not themselves associated with any granitization processes. Other Scottish chloritoid-rocks, such as those in the Dalradian between Aberdeen and Stonehaven, appear to be in a position where great dislocations are necessary to interpret the regional tectonics. These are suggestions that require to be followed up—for the present, I attach the chloritoid rocks to orogenic metamorphism.

I have been shown two developments of chloritoid-bearing rocks in the Transvaal that combined raise a topic of general interest. The first is the chloritoid slates of the Rand, kindly demonstrated to me by Dr. Pelletier and his geological staff of New Consolidated Goldfields. I suggest that these rocks are

formed in the special stress environment provided by shales compressed between stiff competent bands. This suggestion needs testing over the whole Rand but, if it is proved correct, it would support my view that the development of chloritoid takes place in a dislocation-environment—chloritoid is a 'stress' mineral. The second occurrence of chloritoid shown to me indicates that chloritoid is nothing of the kind—(*sui generis* is creeping in again). This occurrence is within the contact aureole of the Bushveld Igneous Complex and was visited under the guidance of Dr. Willemse—it is the well-known locality near Burgersfort in the Eastern Transvaal of andalusite-chloritoid hornfels. There is no evidence here that the chloritoid was formed under conditions any different from those controlling the formation of andalusite: they are both 'anti-stress' minerals of normal thermal metamorphism—(*sui generis* has arrived).

These two occurrences of chloritoid reinforce the opinion that I have advanced repeatedly in other places—the division of metamorphic minerals into 'stress' and 'anti-stress' types has no foundation in the field. All so-called stress minerals occur in non-stress environments, all so-called anti-stress minerals occur in stress environments; stress and anti-stress minerals frequently occur together. The division is dangerously simple and misleading.

I must return for a moment to orogenic metamorphism and its separation from granitization metamorphism. It is as well to realize now that this separation may be one largely of time—there is likely to be an ultimate genetic connexion between the two, as we discuss later. Orogenic metamorphism is an early process in the history of the deformation of the geosynclinal pile, granitization metamorphism is a later process, initiated in the depths of the folded pile and taking time to extend its domain to the higher levels.

It is possible that there are other apparent exceptions to my general proposition concerning regional metamorphism. So far I have regarded the metamorphic rocks as resulting from the transformation of supracrustal rocks, sediments, flows, shallow intrusions and the like. We should, however, at least consider the possibility of certain rocks, now grouped with the metamorphic rocks, having come up from deep zones of the crust where they acquired their present characters. Certain *eclogites*, for example,

may not represent the final stage in the progressive metamorphism of basic igneous rocks but may be derived, as eclogites, from a deep earth-shell. Eclogites are seen to be breaking down into amphibolites, they are transformed into amphibolites by granitization operations; amphibolites, so far as I know, do not pass into eclogites.

It must be recalled that du Toit (1937) considered eclogites to be the result of paramorphic changes and to represent highly transformed and condensed material, neither igneous nor metamorphic.

'They should be regarded as representing the normal lower part of the primitive sial that has experienced regeneration repeatedly. It should not be assumed that they represent surface basics carried down to the depths and there transformed.' (du Toit, 1937.)

This is a view that requires careful consideration. Especially is this the case in such eclogite occurrences as those I have seen in the Jurassic Franciscan in California. Here bodies of eclogites and serpentine occurring in sandstones are clearly in the wrong genetic setting—they can reasonably be interpreted as tectonic pips squeezed up from the depths. The glaucophane-schist associated with these pips may be located, I suggest, in special movement-zones related to their upward migration.

VII. TIME IN GRANITIZATION AND METAMORPHISM

It appears likely from what I have said that an orogenic metamorphism can be provisionally distinguished from a granitization metamorphism. It is apparent that for further advances we must bring the true geological dimension of time into the matter. Geology is not the description of the earth's crust, it is the history of the crust. We have now to consider time in relation to granitization and metamorphism.

I have proposed what I call the *Granite Series* which attempts to relate both time and place in the genesis of the plutonic rocks. From a relatively deep-seated early ancestor are derived the later and higher-level members of the series.

The original earliest and deepest term of the series is provided by the *autochthonous granites*, the granitization granites surrounded by great aureoles of migmatites and metamorphic rocks. Here transformation has worked on the grandest scale and heat and material have been available in great draughts. The Old Granite of the African continent, displayed so magnificently in Namaqualand, Eastern Transvaal and the Rhodesias is on my view an autochthonous granite. In this stage of the Granite Series we see developed over vast areas all the varied phenomena of soaking and permeation, of felspathization and granitization. Variety is to be expected: the materials undergoing granitization are diverse in composition and structure and are of varying resistance; the incoming fluids may be wet or dry, concentrated or diffuse. There is, however, one general rule amidst this variety—the relics of country-rock discernible in autochthonous granites are high-grade metamorphic rocks, maybe of charnockitic type or bearing sillimanite abundantly. When such granites are followed to their margins they pass by more obviously mixed zones into regionally metamorphosed rocks of lower grades—the passage being abrupt when resisters set up barriers against the metasomatizing agents, as I have suggested for Swaziland.

Many of the great autochthonous granitization granites show extensive developments of gneisses of many kinds. Indeed, these granites are the true homes of the real gneisses. Many of the gneisses arise directly by felspathization and permeation with the 'blowing-out' as it were of the schistose texture into the gneissose. Often it can be demonstrated by direct field-observation that other gneisses, especially those with platy layered or streaky structures, result from the slight movement of coarsely felspathized country-rocks. I have for example seen both on the Umgeni in Natal and in Namaqualand excellent exposures that demonstrate the onset of plastic flow in coarse felspar-porphyroblast rocks with the deformation of the felspar ovoids. Much of the granitization granite itself is of course gneissose and foliated due to the differential persistence of varied country-rocks and to the differential soaking capacities of different layers. With the exception of the restricted class of gneisses that result from the cataclastic cold shearing of coarse solid crystalline rocks, the great rock-group of the gneisses is firmly attached to the plutonic processes.

Their variety is exactly what is to be expected from plutonic operations.

It is obvious from the nature of granitization and the diversity of rocks upon which it acts that portions of the products may have different plasticities. More mobile parts will come unstuck from their settings and thus present intrusive relations to their less plastic neighbours. In such mobile portions, the ghost stratigraphy and structure of the autochthonous granites become disturbed; there are many fine examples of this in Donegal and Namaqualand. As I have said already, there appears to me to be a correlation between the shape of the cupriferous bodies and the mobility of the adjacent migmatites in the O'Okiep copper field of Namaqualand—a correlation explicable only if these bodies are resisters and not post-migmatite intrusions.

What is seen to occur on a small scale in autochthonous granites can reasonably be expected to occur on a larger scale, then producing the next term in the Granite Series—the *parautochthonous granites*, higher and later in the series. By mobilization, certain portions of the autochthonous granites begin to part company with their genetic setting and move higher in the crust or, more usually, into tectonic domains of lower pressure. The resulting parautochthonous granites show variable marginal relations, in some places migmatitic, in others characterized by an aureole of thermal type. This movement out of the migmatitic-metamorphic setting may continue till the genetic ties are completely severed and *true intrusive granites* emplace themselves in higher levels of the crust, maybe as magma but more likely as migma. The final term of the Granite Series is represented by the high-level *plutons*, intrusive into non-plutonic regions late in the history of the orogen concerned. (The nomenclature itself appears to be becoming *sui generis*.) These late terms are surrounded by aureoles of thermal metamorphism and they make their way by 'plain pushing and shoving' up through the softened-up country-rocks. I have already given a sample of the evidence for this interpretation in connexion with the Cape plutons. The plutons are the domain of the *Granit-tektonik* of the Cloos school and, as their emplacement produced considerable folding and distortion in the country-rocks surrounding them, they came in as almost solid 'dead' bodies. It must be emphasized once again that the

metamorphic history of the country-rock envelope around the plutons is of the greatest significance in the interpretation of their intrusion history. The breaking-down and distortion of early contact-minerals with continued uprise of the almost solid plug can often be observed, as for example around the Ardara granite body in Donegal.*

The members of the Granite Series have their own individual properties and can be recognized from them. On this basis, I have mentioned and interpreted already several granite bodies occurring in the Union—there remains one more for consideration, the Kaap Granite of the Barberton area. I was introduced to this granite by Dr. van Eeden, whom I thank for much enjoyable discussion, happily still continuing. It seems to me that the Kaap Granite has all the characters of a pluton—shape, granite-tectonics and the rest. According to current Survey opinion, however, it is the oldest granite of the area, older than the Old Granite (= Nelspruit migmatite), and against it the metamorphic rocks have been folded as against a buttress. I suggest, more or less humbly, that this opinion might be reconsidered; it might be possible that the Kaap Granite is a young pluton in the Old Granite migmatites and its emplacement has caused the folding of the adjacent metamorphic rocks; we must wait and see.

We thus find a variety of granite bodies and types associated with appropriate developments of metamorphic rocks. As I have so often stated, there are granites and granites. I suppose that a man who walks in the middle of the road must expect to be knocked down by traffic from the rear and the front simultaneously; something of the kind is happening to my geological self at this present time. In the same part of the Report of the International Geological Congress of 1948, my proposal of granites and granites is assailed

* Now that I have invoked mobilization, I may perhaps be permitted an irrelevant aside. This concerns the celebrated Dwars River occurrence of chromite bands in anorthosite of the Bushveld Igneous Complex, described by A. L. Hall (1932, p. 338, Fig. 33, Plate xxvi). *On the field-evidence*, the chromite bands are intrusive into the anorthosite—but the same chromite bands become broken up, distorted and distributed through the same anorthosite. It seems to me that a possible solution of this problem is mobilization of parts of the anorthosite after the intrusion of the chromite with consequent brecciation and disturbance of the chromite bands. There is available here a nice study of detail. I thank Dr. J. Willemse for showing me these entertaining exposures.

from the magmatic side by Eskola (1950) and from the dry transformist side by Roubault (1950)—it is my experience, nevertheless, that the question of *either-or* is most often correctly answered by *both*.

No autochthonous granite shows a progression over the whole Granite Series to the final pluton, any more than one nebula shows the whole sequence of nebular configurations. In an orogenic belt there is often a spreading-out in the direction of the main tectonic drive of the different members of the series. It is important to remember that at the present level of exposure a pluton emplaced in a migmatitic autochthonous granite cannot be directly related to its immediate surroundings; it has come from a deeper granitization centre. But in the one orogen, granitic manifestations are reasonably to be connected.

We may now resume the consideration of the relations between orogenic metamorphism and granitization metamorphism. On my reading of granites, they are closely associated with the later stages of orogeny. They are not derived directly from basic magma, this appearing in quite another setting, either during the filling of the geosyncline or during the formation of tensional fissures later than the orogeny. It may be suggested that the first onset of orogeny folds the upper portions of the geosynclinal filling, and there produces what we have called the orogenic metamorphism, normally of low grade. In the deeper portions of the geosyncline thus compressed, granitization takes place on a grand scale. I have no time tonight to consider in detail the provenance of the granitization fluids. Their derivation by expulsion from a granitic magma or by differential anatexis of the heated basal sediments raises many difficulties. Large-scale granitization requires considerable draughts of alkali-silicate. Whether these are thought to come from a granitic magma or from heated sediments, they should leave behind as residuals queer granites or equally queer sediments; if these are indeed formed we see little of them at the levels of exposure available to us. I suggest for discussion that we seek the ultimate source of the granitizing fluids in crystallizing simatic material below the site of the geosyncline. In any cycle involving sedimentation-orogeny-granitization, there is always a relative loss of sodium to the sea. To maintain what seems to be the standard pattern, there must be an accession from outside of sodium in

each cycle. The simatic layer may be its source. This suggestion needs a great deal of examination before it can amount to more than a guess, but the results may be well worth the trouble. But whatever the ultimate origin of the granitizing fluids may be, they can be considered to move higher into the folded belt to cause autochthonous granitization. With continued development of the fold-belt, later members of the Granite Series reach higher into the structure—the granitic manifestations ending with the emplacement of the dead plutons, highest and last.

The degree of dissection of an orogenic belt naturally controls the amount of granitization revealed within it. It is a problem for the future to decide whether the age of the orogeny may be correlated with some diversities in the style of granitization and the development of the Plutonic Series. Without abandoning uniformitarianism, I have formed the impression that the younger fold belts display quicker transitions between the three terms of the Plutonic Series and that, on the whole, the products are of finer grain. I admit that my field-experience of young fold-belts is practically limited to that of the Cascades of Washington and Oregon and is little enough—there may be many exceptions and many explanations. But it seems to me that whilst each orogen produces the general pattern, the products in detail are again *sui generis*.

I have mentioned uniformitarianism. We must cling to this great doctrine as long as we honestly can. This advice may be particularly relevant in the study of Basements such as yours. If we relax, we may easily become anarchic—the deeper geology may pass into the higher lunacy. When we interpret the greater part of the Basement as the product of granitization, we believe that we can recognize there normal sedimentary and magmatic rocks that are similar to those being formed today. Our oldest autochthonous granites of the Basement require a sedimentary terrain from which the granites have been formed. We are never within sight of a beginning.

VIII. A NEW CLASSIFICATION OF ROCKS

Because of the close genetic connexion between granites,

migmatites and metamorphites, I have suggested that we get a new orientation on rock classification. It is unwise, for example, to call a rock igneous unless we can show that it has consolidated from magma. Further, such a term as igneous, used in the current sense, obscures what I consider to be fundamental geological differences between rocks. We should be geologists first and physical chemists, petrographers and physicists later, if we care to. Looking at the so-called igneous rocks geologically, we can discover a fundamental difference between the granitic and basaltic parts of them. This geological difference was clearly seen a century ago, but became submerged beneath the petrographical classifications of the late nineteenth century—it still remains partly submerged. These two commonest rocks of the crust differ completely in their geological settings; the granites are the dominant intrusives, the basalts the dominant extrusives; granite is associated with orogeny and compression, basalt with stable belts, with the making of the geosyncline or with late, post-orogenic, tension.

In recent times, this dichotomy has been explored by Kennedy and Anderson (1938). Kennedy has proposed the separation of the igneous rocks into two groups, the Volcanic Association and the Plutonic Association, representing two distinct and apparently independent expressions of magmatic activity, originating in different parent magmas and evolving by different processes. The Volcanic Association is derived from primary basaltic magmas by melting of the sima, erupted by relatively narrow dyke-like fissures directly to the surface where it appears as the gigantic lava-piles; a portion may not reach so far and freezes as dykes and sills and laccolites. Differentiation by crystal settling is valid in the group and its magmas are dominantly basic. It is important to realize that the Volcanic Association includes all intrusives that are genetically related to the basic volcanic activity and originate in the same magmatic source. The gabbro-diabase-basalt group cannot therefore be separated in any genetic classification. It may be somewhat of a shock to you to hear me refer to your gigantic Bushveld body as volcanic rock—but genetically it is; because its magma did not happen to reach the surface is no reason for attaching it to other bodies such as granite that never reach the surface anywhere. I cannot subscribe to the recent proposition that the Bushveld body is of metasomatic origin. I believe from

the world-wide evidence that basaltic magma is available in enormous quantities to be emplaced quickly and directly at high levels in great segments of the crust. By all means let us reorient ourselves with regard to the igneous rocks, but I shudder at the complete extinction of that ancient group.

With this digression over, let us return to the second of Kennedy's associations, the *Plutonic Association* which he considers derived from the primary granodiorite shell by remelting, the magma thus produced penetrating slowly upwards, accompanied by a wave of granitization and migmatization of the country-rocks. The requisite magma is supplied by remelting of the sial which is possible only within the orogenic zones of the crust where tectonic thickening brings the base of the sial within range of fusion.

Even if all granites were igneous and magmatic, there is a reasonable opinion for separating them from the gabbro-basalt family. But this is contrary to the magmatic school, and Bowen has chided me for rejoicing in freeing granite from a parental basaltic magma. Let us suppose a granite magma is required—how is it produced? Since it is likely that there are two earth shells corresponding in their general compositions to the two commonest rocks of the crust, granite and basalt, then I hold it reasonable to derive a primary granite magma from the sial and a primary basaltic magma from the sima—two shells, two magmas. Granite magma might arise by pure melting by depression of the sial in the orogenic belts, or by heating up by uprising basaltic magma, or by partial melting and squeezing-out of the resultant liquid as in anatexis. All these propositions seem reasonable if you require quantities of granitic magma. But the thorough-going magmatists will not have this at all—granitic magma is a secondary magma produced by crystallization differentiation of a primary basaltic magma. Now I should like to make it perfectly clear that I see no objection to deriving granitic magma from primary basaltic magma. Bowen has done it in a crucible and nature has possibly done it at the tops of some thick basic sheets, though much of the stuff called granite in such positions is more likely granophyritized roof-rock. But these are pocket examples when compared with the great granitic complexes. On many grounds—of composition, volume, and mechanics in particular—I reject the monomagmatic view of the igneous rocks. As I have said before granites and

basalts are different things—it is comforting to find my friend Eskola, a reasonable magmatist, saying recently 'Read's statement that granites and basalts are different things . . . is a true and fruitful idea, now ripe to be generally pursued.' (1950, p. 10.) I should add that in the next paragraph, as I have already said, Eskola goes on to disagree completely with my view that there are granites and granites, but I value his approval of the resurrected view that granites and basalts are different things.

The granites, as we have seen, are associated with compression and orogeny—the basalts appear in the filling of the geosynclines or rise up tensional fractures after orogeny has ceased. The two groups record fundamentally different events in earth history. We agree I trust that there is the closest genetic connexion between the granitic, migmatitic and metamorphic rocks, that my Plutonic Series is real and valid. They make one group in my proposed new classification which I now present:

1. Neptunic*—the sedimentary rocks, dominantly marine.
2. Volcanic—effusive and associated intrusives: dominantly basic, magmatic, igneous: non-orogenic.
3. Plutonic—the granitic, migmatitic and metamorphic rocks; orogenic; the great granitic complexes, the gneisses and schists.

If this proposal has any value, then there is possibly required a reorientation with regard to the genesis and classification of ore deposits. But that is a matter on which I have no knowledge—I leave its exploration to any interested parties present here tonight. But I do suggest that concentration by subtraction is not ignored.

IX. THE GREATEST PROBLEM IN GEOLOGY

My last topic concerns the greatest single problem in our science—the origin of the discontinuous granitic shell, separated in the continental masses and buoyed up on the continuous basaltic shell below. When I have finished, I hope you will agree that this

* Perhaps Neptune would not be the most suitable choice as the patron of South African Sedimentary Geology.

topic is not so unrelated to this discussion as it may seem at first sight.

It is the standard belief that the early earth had a continuous granitic shell over a continuous basaltic shell—a result either of crystal-fractionation or of liquation. The great problem is to produce the present discontinuous granitic fragments.

It has been suggested, by Daly for example, that during the immense span of Pre-Cambrian time, the original continuous granitic shell has been buckled-up and thickened by Archean orogenies. We who live on the continents become obsessed by their size and importance—it seems to me that the granitic cover produced by unrolling the continental masses would be exceedingly thin. Besides, we find the deepest parts of the granitic shell accessible to direct observation to be made of granitized sediments and not of igneous granite. Sir George Darwin long ago cut the knot by proposing that some portion of the granitic shell was removed when the moon escaped from the earth. For this to be possible, the shells should be liquid at the time of escape, a suggestion not favoured by the magmatic school. I am told, too, that Darwin's proposal is not acceptable to astronomers who now regard the moon as having been nearer the earth but not directly part of it.

Perhaps the grand problem as it is currently stated cannot be solved—and it may be worth while stating it differently. I suggest that we do not think of a continuous granitic shell subsequently made discontinuous, but of continental granitic blocks discontinuous from the start. As Walter Bucher has well put it: '. . . many of us are beginning to think that instead of asking: "Why is granite absent from the oceans?" we should be asking: "Why is granite present in the continents?" ' As I have just said, the basements are largely granitized sediments and I propose that the continental blocks have been built up by continued granitization of the earliest sediments. The formation of these sediments may have taken place in various ways. Rittman has suggested that the granitized materials condensed from the early atmosphere. I would like to suggest that the early earth had a solidified outer basic layer (the earth is a very basic body indeed) which had certain slight irregularities perhaps caused by the irregular accumulation in patches of granophyric differentiates, similar to those at the tops of thick basic sheets. Denudation of this

material could be followed by the granitization of the resultant sediments—the granitizing fluids arising as a consequence of the crystallization of further quantities of basaltic magma. Once the process starts it will go on, with repeated granitization building up sialic rafts. As these grew, they came up out of the hitherto universal ocean by isostatic adjustment. We have then no problem of a continuous granitic shell made discontinuous—it began like that. I conclude these not too sedate remarks by leaving this solemn thought with you: *It may be that it is only by the grace of granitization that we have continents to live on.*

VIII

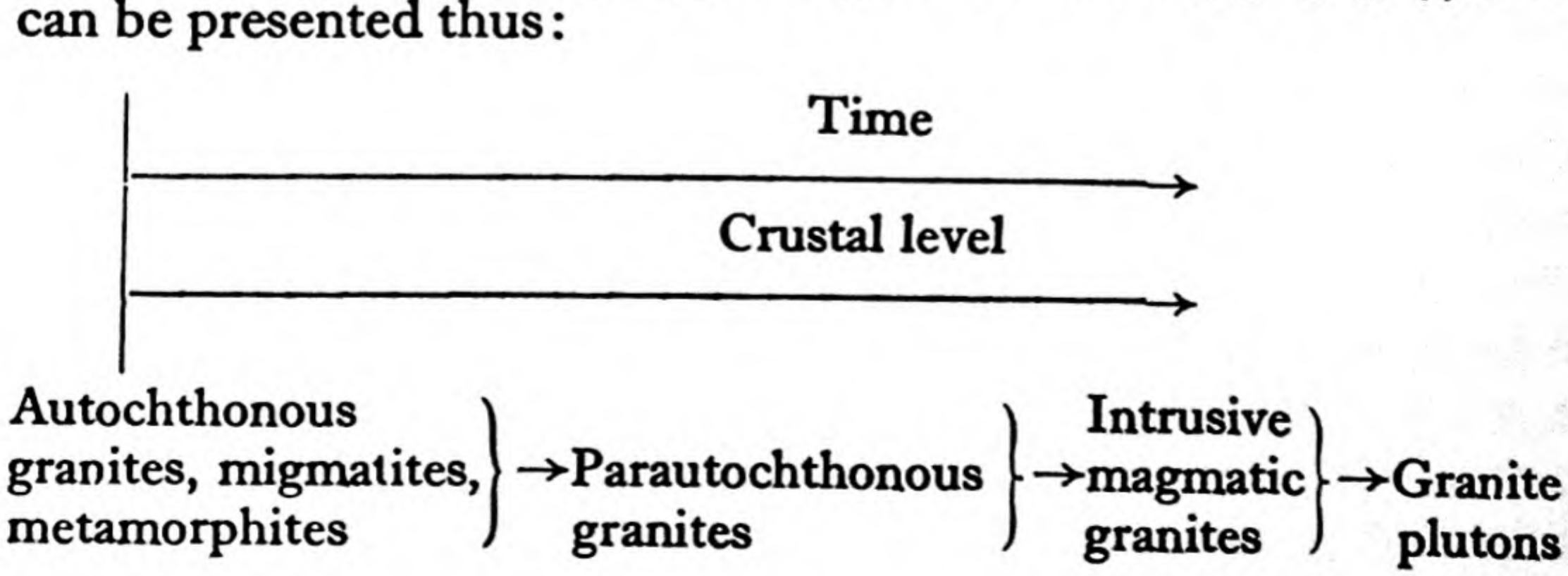
GRANITE SERIES IN MOBILE BELTS

1955

INTRODUCTION

POSSIBLY the most creative development in petrogenesis is that based upon the realization that a given rock is not formed by chance at an accidental time or random place, but that it records precise stages in crustal evolution at definite times at circumscribed places. Petrogenesis has its role in Historical Geology as much as Stratigraphy. This is particularly true for the crystalline rocks. It is becoming clear that the composition, setting, and form of an igneous rock are all specific and related qualities expressing the relevant magmatic and crustal history. It seems that judgements on the origin of crystalline rocks drawn from a consideration of their time and place relations—that is, of their geology—are greatly to be preferred to those based upon any other study. This applies especially to the granitic and metamorphic rocks; there are granites of various kinds, and they are related to various settings and events.

These granites of various kinds make up the *Granite Series* (Read, 1948*a*, p. 17; this volume, p. 192; 1949, pp. 143–8; this volume, pp. 324-37; 1951, pp. 20–3; this volume, pp. 363–7) which can be presented thus:



This scheme is based on the opinion that, though there are granites and granites, all the members of one series are genetically related; they are in fact serial. The case for the Granite Series will be re-developed in later pages, but, for ease of proceeding, it will be advantageous to assume that the series is real—such is indeed implied in the title of this essay, which also includes a second assumption. It is a matter of historic observation that granites, gneisses, and metamorphic rocks—what I have called the plutonic rocks (Read, 1944, pp. 91–93; this volume, pp. 162–5; 1948*b*, p. 156; this volume, pp. 195–6; 1951, pp. 4–5; this volume, pp. 342–4)—are especially abundant in the mountain, fold, or mobile belts. As the old aphorism has it, each mountain chain has its gneisses. Some may hold that this is purely fortuitous, but most will agree that there is the closest correlation between the type of plutonic activity and tectonic style; a few may even hold that certain classes of mountain crystallines are simply the more mobile parts of the mobile belts. We propose therefore that the Granite Series is an expression of events in the mobile belts.

Accordingly, in this inquiry most attention will be directed to those parts of the crust where the maximum appearance of granitic rocks seems to coincide with greatest mobility. Granitic rocks become commoner in deeply eroded areas, and such areas are structurally troubled and complex. In these basements, this term indicating crystalline rocks lying below a sedimentary cover, we may decipher a pattern of events that may give a clue to those occurring at higher levels and later times. Besides, the basement does not remain untouched by the mobility of its cover, and the question arises as to whether its reaction is passive or active. In any exploration of the basement in a mobile belt, a datum plane or a key time is of the utmost importance; here the history of the contact surface between basement and geosynclinal pile requires the closest examination.

Whatever the origin of granitic rock may be, it is conceded that the cause is a deep-seated one. Granitic complexes in well-exposed basements may thus provide models of certain terms of the Granite Series, especially the earlier and deeper ones. In the higher levels of the mobile belts, granitic material seems to appear late in the orogeny, and consideration must be directed to the effect of subsequent structural evolution on the form, emplacement,

lithology, and composition of the granites late and high in the series.

This inquiry therefore has two main themes—the first is concerned with mobility connected with the appearance of granitic rocks in the basements, the second with the appearance of granitic rocks connected with mobility in the geosynclines. After these themes have been explored, a more fundamental problem arises, that of the relation between events in the geosynclinal pile and in and below the basement. The conclusion is reached that the whole drama of plutonic activity and orogeny in the mobile belts depends upon events taking place deep in the simatic layer.

MOBILITY AND GRANITIZATION IN THE BASEMENTS

The foundations of the mobile belts can be studied in two ways —one direct, the other indirect. The first is by direct observation of basements, with detailed investigations of their petrology, tectonics, and plutonic history; the second is by indirect deductions from the nature of later sediments as indicative of crustal movement and from the form and type of intrusive bodies in these sediments as recording magmatic or related evolution. It is clear that the first method is more likely to lead us to the prime cause underlying any relation between crustal movement and granitic phenomena. I propose therefore to deal with field observations in the basement at some length, particularly as my school has been busy with such matters for a dozen years, or more.

It is necessary to discover well-exposed regions where the modification of the basement can be established beyond question, and where the appearance of granitic rocks can be correlated with deformation and metamorphism, the principal orogenic operations recorded here. Many such regions are known in deeply eroded areas, though unfortunately their positions in orogenic belts are not everywhere clear. A few examples are presented to demonstrate what appears relevant.

From Finland, where the devoted labours of Sederholm and his successors have laid bare the astounding complexities of the Finnish Archean, the writer selects from the innumerable studies three for comment, leaving that of Eskola on mantled gneiss domes for a

later page. Wegmann and Kranck (1931; Kranck, 1937) demonstrated the kinematic aspects of migmatization and granitization especially in the Hangö district. Here, soaking of the country-rocks to give migmatites developed a potential mobility in the products which ultimately were able to move and so produce intrusive granitic bodies. In migmatized regions, the movement directions are not constant over any great distances, the styles being controlled by different degrees of plasticity, the migmatites becoming 'wild'. Folding or movement and granitization were contemporaneous. Edelman (1949), in the Gullkrona basin, has demonstrated the complicated structures in migmatites due to variation of the plasticity of different rocks under different degrees of granitization; the fold axes have almost any direction. Under plastic deformation, the mobilized granitic rocks rose and expanded to produce the Gullkrona dome, with arcuate structures in the more plastic rocks. In the classic Orijärvi region, Tuominen and Mikkola (1950) constantly invoke plastic flow as the dominant plutonic operation. The onset of mobility with granitization has been proposed by many other workers in Fennoskandia; some of these contributions will be examined later.

In their investigations of the beautifully exposed crystalline complexes of Greenland, Wegmann (1935*b*; 1938) and Kranck (1935) reinforced their Finnish conclusions. Wegmann (1935*b*) described the elegant flow structure and flow folds culminating in intrusion consequent upon the mobilization of migmatized rocks; he (1938, p. 141) decided that the granites are the metamorphic products of the orogenic cycles. Kranck (1935, pp. 92–3, 110) considered that every gneiss terrain investigated shows one strong granitic injection associated with strong orogenic action; metamorphism, granitization, and orogeny are all coeval. Recent work in West Greenland by Noe-Nygaard and Berthelsen (1952) brings out very clearly the relation between tectonic style and granitization. Two metamorphic facies are involved: the granulite-facies rocks show a simple style of folding, whereas the amphibolite-facies rocks which have undergone granitization are arranged in domes and overturned flow folds, indicating the greater plasticity and mobility of these rocks.

Many more examples of mobility in the basement could of course be given from all over the world—I can only cite those from Central

Africa (Temperley, 1938) and South Africa (Read, 1951, pp. 12, 20; this volume, pp. 354, 364).

Determination of the style of these movements in the basements is often difficult because we are not concerned with the folding of originally flat beds. As was done in the investigations listed above, modified and unmodified areas can be compared, but better still is the use of the deformation and transformation of originally straight-edged dykes. This method has been employed by Ramberg (1951) in West Greenland, Kranck (1953) in Baffin Island, and especially by Sutton and Watson (1951) in the Lewisian complex of the North-West Highlands of Scotland.

Ramberg (1948, 1951) dealt with the gneiss complexes later discussed by Noe-Nygaard and Berthelsen. He showed that there were two deformation periods separated by a tensional episode of diabase-dyke intrusion. The granulite-facies rocks of the older orogeny, together with the diabases, provided the material on which the second orogeny worked. The diabases were folded, recrystallized into amphibolites, boudinaged, pegmatized, and metamorphically differentiated. In the early stages of the second deformation, they partly controlled the structural style, but this control gradually weakened. The younger complex of amphibolite-facies rocks is thus reactivated older complex plus deformed diabase material. According to Ramberg, no magma of palingenetic or anatectic origin was involved, the operative process being a metasomatism brought about by the migration of elements, preferentially squeezed out from the deep granulite facies and moved upward to give the granitization of the shallower amphibolite facies. Whatever the precise mechanism may have been, we can agree that mobility and transformation coincided. In Baffin Island, Kranck (1953) illustrates the progressive deformation of mafic layers on migmatization of their enclosing rock—the production of ladder-vein structure, then boudinage structure, and finally bead structure. The granitized country-rocks show the wild folding typical of such complexes—recumbent folds, crenulations, flow breccias, and other evidences of plastic deformation. Kranck contrasts this tectonic style characteristic of the migmatites of the amphibolite facies with that of the granulite facies; the granulite facies is dominated by flat parallel foliation or flattened recumbent folding. The contrast arises from the presence

in the migmatites of a fluid granitic phase, whereas the granulite facies was relatively dry. Again is illustrated the connexion between mobility and granitization.

Nowhere can this same connexion be more clearly demonstrated than in the Loch Torridon and Scourie districts of the North-West Highlands of Scotland, investigated in great detail by Sutton and Watson (1951). In these regions of the Lewisian Gneiss Complex, two periods of metamorphism, migmatization, and deformation, separated by a tensional diabase-dyke interval, have been established. In the Loch Torridon area, Sutton found that an amphibolite-facies assemblage, with a north-easterly foliation, was invaded by diabase dykes and the whole was later subjected to a second metamorphism and migmatization during which a north-westerly foliation was impressed on the new complex. This second act can be followed in detail by observations on the attitude and modification of the mafic dykes: shear belts were first formed, but later the crystalline wedges themselves were activated so that they reacted to the new tectonic stress. With the final onset of rigidity, the lingering stresses were taken up along mylonite belts. During the attainment of the new tectonic style, large-scale plastic flow was made possible by migmatite soaking. In the Scourie area, described by Watson, the charnockitic rocks of the first plutonic episode provide evidence of flow and of mobility of the felsic components. These gneisses, together with the diabase dykes, were again metamorphosed and migmatized with the establishment of the amphibolite facies. Watson mapped zones, from south-west to north-east, marked by increasing plasticity; in the south-west the old complex failed along shear belts, in the north-east the rocks yielded by plastic flow. The migmatite front approached from the north-east and the plastic zone 'is clearly due to the high degree of metamorphism and to the immense amount of veining and soaking by granitic material that had taken place' (Sutton and Watson, 1951, p. 290). A new north-westerly foliation characterizes the later complex. The relation between granitization and plasticity is represented in Fig. 19. From the discussion that followed the reading of Sutton and Watson's paper, two abstracts may be made. Shackleton emphasized the 'intimate spatial relation between migmatization and plastic deformation', a statement which the writer endorsed by calling attention to this

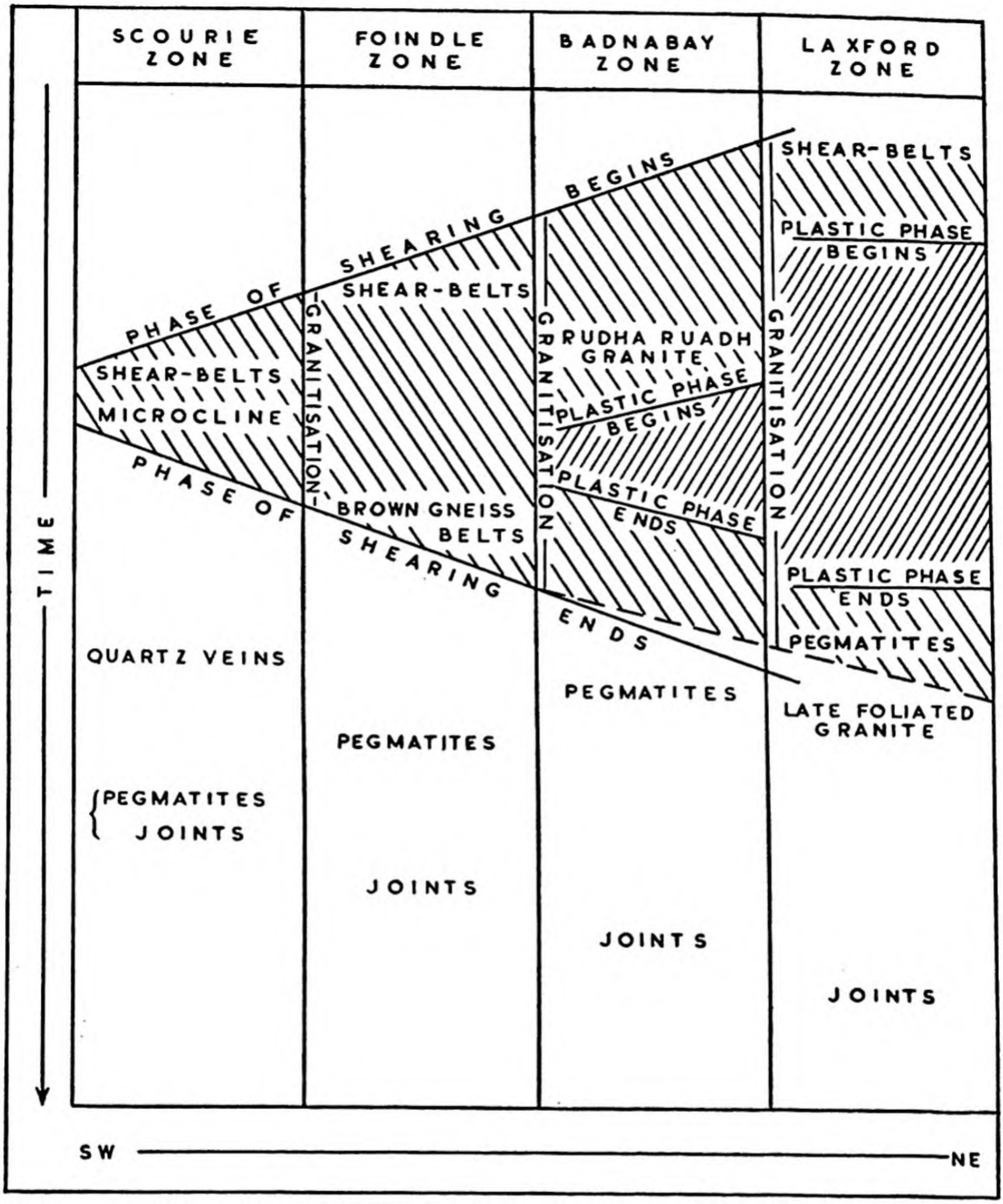


FIG. 19.—Sequence of Laxfordian events affecting the Scourian basement, North-West Highlands of Scotland.

Each vertical column contains the history of one zone, and contemporaneous events in different zones appear at the same level. (After Janet Watson, *in* Sutton and Watson, 1951.)

'demonstration of the increase of mobility of segments undergoing migmatization'. As Shackleton put it, 'the migmatite fronts and the flowage fronts were essentially coincident'.

We are now in a position to make certain general conclusions from this sampling of the basement. From both comparative studies and geometrical observations, it is clear that parts of the basements can acquire a new structural style and change from a relatively rigid to a plastic condition. Pre-existing components such as sedimentary relics and resisters (Read, 1951, p. 7; this volume, p. 347) in general take up new attitudes, but, though it is certain that the affected basement flows, it is just as certain that most of it never approaches a liquid state. This new condition is not entirely dependent on pressure-temperature changes since it is usually patchy in development; there must be as well a chemical transformation—that is, a granitization. There is a close correlation between plasticity or mobility and the extent of permeation, soaking or migmatization. The granitic rocks produced in the plastic areas are of a special kind. They do not make large coherent masses—if such do occur in modified basements they can be shown to be late—but they are diffuse, to use Raguin's excellent term (1946). They are moreover accompanied more or less closely by migmatites and high-grade metamorphic rocks usually of the amphibolite facies. This whole assemblage of granitization granites, migmatites, and metamorphites, the writer's Plutonic Rocks, is developed on the largest of scales. Thousands of square miles of such complexes are known in all the continents (Read, 1951, pp. 4–5; this volume, pp. 342–4). These diffuse granitization granites show what the deepest and earliest term of the writer's Granite Series, the autochthonous granites, may have been like; we return to this aspect later.

REACTIVATED BASEMENTS IN SUBSEQUENT OROGENY

The surface of unconformity between the basement and the overlying sediments gives an excellent datum plane for assessing subsequent movement of the foundation. Its study shows that reactivation of the basement commonly occurs during later orogenic movements. This study has been the special concern of Eskola (1949; 1951; 1952) in his demonstrations of the mantled gneiss domes of Finland.

In the Karelid mobile belt of East Finland, there occur a

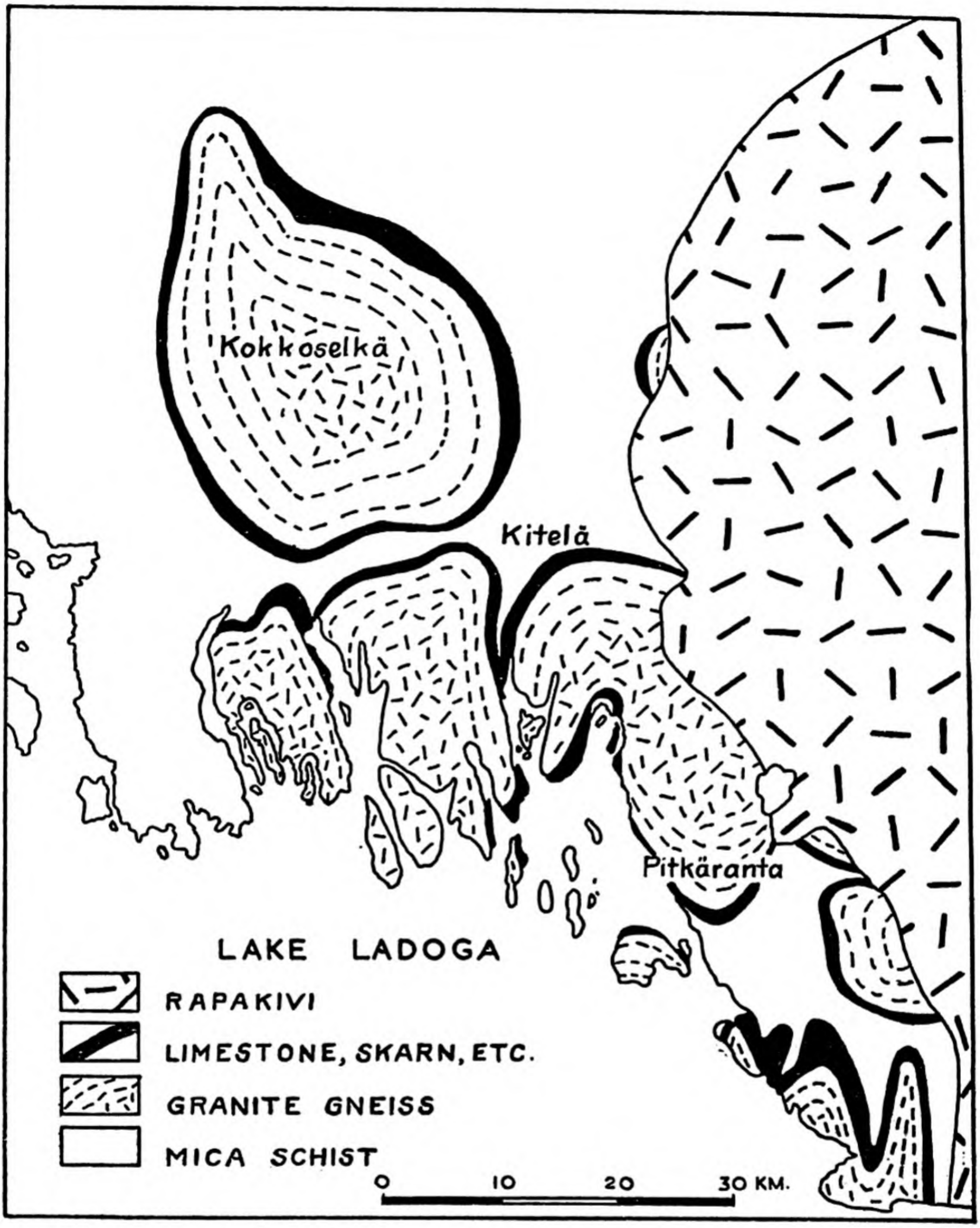


FIG. 20.—Mantled gneiss domes, Pitkaranta, Finland. (After P. Eskola, 1951, Fig. 1.)

number of remarkable granitic gneiss domes, surrounded by regular successions of strata dipping outwards from the domes (Fig. 20). In a few the succession begins with a basal conglomerate which contains pebbles of the underlying dome gneiss. *The gneiss of the*

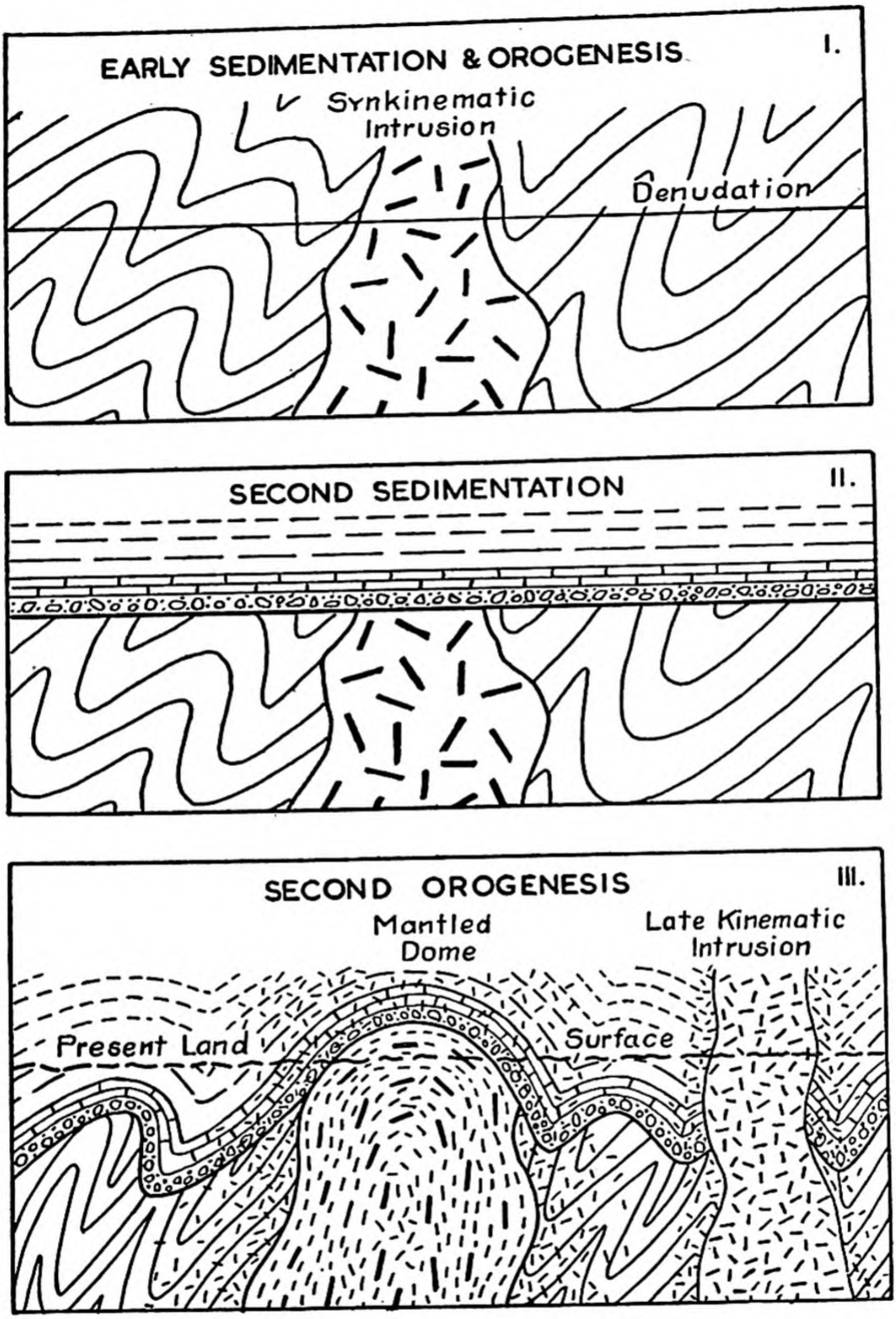


FIG. 21.—Sequence in the production of mantled gneiss domes. (After P. Eskola, 1949, Fig. 7.)

dome is therefore intrusive into the schists of which it is the basement. Eskola's explanation, shown in Fig. 21, is that a synkinematic granite of the earlier Svecofennid orogeny has been reactivated by metasomatic granitization during the later Karelid folding; 'it seems as though a plutonic intrusion received new life when a new orogeny befell' (Eskola, 1949, p. 468).

Eskola interprets the granitic domes of Maryland (e.g., Cloos and Broedel, 1940) and New Hampshire (e.g., Billings, 1945) in the Appalachian belt along similar lines. The dome rocks, Baltimore gneiss or Oliverian magma series, were originally granitic intrusions belonging to a Pre-Cambrian or Taconic orogeny that have been reactivated in Appalachian times by metasomatic

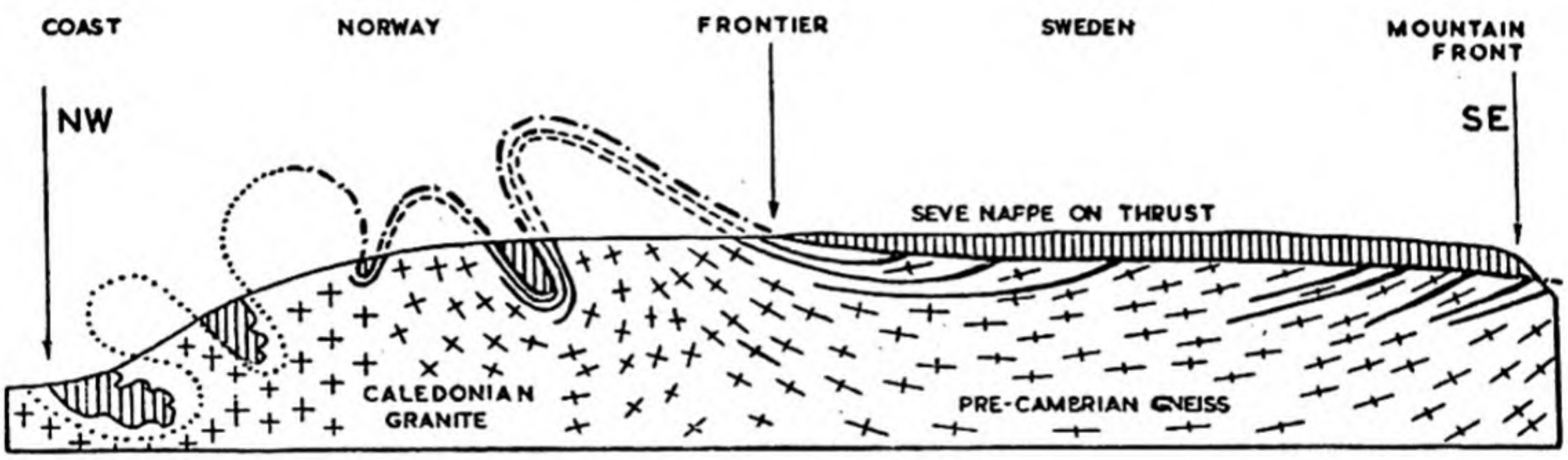


FIG. 22.—Hypothetical section across north Scandinavia. Showing the production of Caledonian granite from Pre-Cambrian gneiss. (After G. Kautsky, 1947.) Length of section about 200 km.

granitization. Eskola also discusses analogous occurrences in Scandinavia and the Alps. We may now consider these.

Kautsky's (1946, 1947) interpretation of the development of granitic rocks in the Caledonian belt of Northern Scandinavia is summarized in Fig. 22. In Sweden, the great Seve Nappe of metamorphosed Lower Palaeozoic sediments was thrust eastward over the Pre-Cambrian basement gneisses. Westward the basement became first mechanically disrupted, then metasomatically recrystallized, and finally mobilized into intrusive granites that invaded the Seve Nappe in great westerly directed fold cores. As Eskola remarks (1949, p. 473), 'As granitization is intensified, mantled gneiss domes pass gradually into magmatic masses, or plutons, that by themselves would be interpreted as laccoliths, anticlinal batholiths or some other type of intrusive masses.'

Folding, granitization, and mobility go hand in hand. The same granitic rock is Pre-Cambrian basement in Sweden and Caledonian intrusions in Norway. Similarly, Holtedahl (1952*a*, 1952*b*) has shown in the Opdal region that the basal gneisses became plastic by Caledonian granitization and are represented in an activated state in the Nordland granite masses. These are, of course, demonstrations of the Granite Series.

Opinion is strengthening that the Hercynian crystalline masses in the Alps have been subjected to Alpid granitization and during the orogeny have not behaved in passive fashion. Twenty years ago, Reinhard (1935) proposed the mobilization of the crystalline basement in the Tessin, and Bucher (1933, p. 181) drew attention to the plastic behaviour of the crystalline core of the Aar massif. Bucher (pp. 199–200), basing his opinion on both Alpine and non-Alpine examples, enunciated his Law 23: 'the structure of welts proves the existence of differential movements in the crystalline cores which approach the nature of "plastic" flow. The degree of plastic behaviour within the crystalline core increases with depth.' Returning to the Alps, we have time to mention only Exner's (1947, 1948, 1949) recent detailed work on the Hohe Tauern gneisses where he establishes the reactivation of the basement during Alpid orogeny and decides that Alpid granitization is widespread. Still more recently, Nabholz (1953) has described the plastic deformation and mobilization of the granitic cores of the Pennids.

From this brief survey we conclude that the basement is not passive in orogeny. It becomes reactivated, capable of movement. Doming, uprise, and true intrusion are all possible consequences of granitization contemporaneous with orogeny. This conclusion is independent of the mechanism of the granitization and of the sources of the granitizing fluids. It holds whether the process is one of differential anatexis (Eskola, 1932*c*, 1933) of basement constituents or of the introduction of emanations, ichors, or anything else from deeper down or from either a magmatic or non-magmatic source. In any process, the basement becomes permeated, lubricated, and mobile; it may leave its roots and become intrusive.

INFRASTRUCTURE AND SUPERSTRUCTURE

From the foregoing presentation of the contrasts between the plastic migmatitic portions of the basement and the rigid non-migmatitic portions, we can pass to the consideration of more general concepts concerned with the differing tectonic styles of the deeper and the shallower parts of a mobile belt seen as a whole. These concepts, owing to Wegmann, Backlund, F. E. Suess, Demay, Raguin, and others, dwell upon the modification of or interference with the tectonic happenings in the lower part of a structural edifice by magma or migma. As the writer has discussed them in some detail elsewhere (Read 1940, pp. 241–2; this volume, pp. 28–30; 1944, pp. 68–73; this volume, pp. 157–61; 1948*b*, pp. 168–9, 197; this volume, pp. 213–4, 257; 1949, p. 128, 134–143; this volume, pp. 301, 313–24), he limits himself here to Wegmann's synthesis.

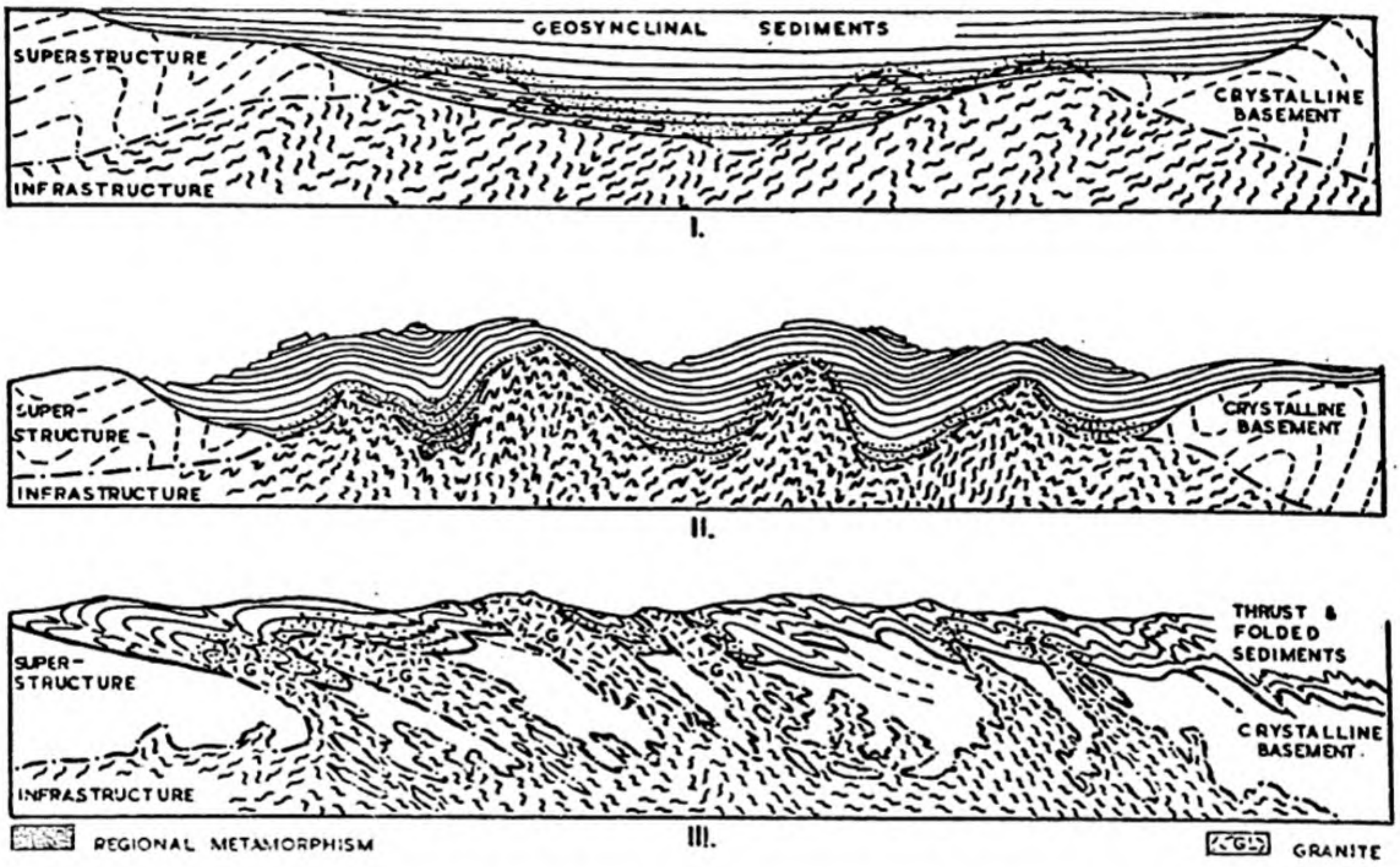


FIG. 23.—Relations between infrastructure and superstructure in different mobile belts. (After C. E. Wegmann, 1935*a*.)

Wegmann (1935*a*, 1935*b*, 1936, 1947, 1951) distinguishes

between the non-migmatitic superstructure (*Oberbau*) and the migmatitic infrastructure (*Unterbau*). Infrastructural conditions arise by the advance of the migmatite front from below to involve both basement and geosynclinal filling alike; the transitional zone between the infrastructure and the superstructure is the domain of regional metamorphism (Fig. 23, I). Depending upon the style of the mobile belt, especially on its degree of cross-shortening, the permeated infrastructure moves into the superstructure as elongated domes in the compressed arches of the structure (Fig. 23, II); in these regions of intense metamorphism and metasomatism, mobilized bodies move still higher and appear as intrusions and diapirs. With further decrease in the cross-section of the mobile belt, the infrastructure becomes thoroughly active and presses as tongues into the superstructure, ultimately appearing at higher levels as 'magmatic' granites (Fig. 23, III). With extreme cross-shortening, the infrastructure and the transitional zone flow into the superstructure in great folds. As we have already seen, examples of these stages are supplied by the Svecofennids and Karelids of Finland, the Caledonids of Scandinavia, and the Alpids. In general, the superstructure is the site of overthrusts, the transitional zone of great nappes, the infrastructure of steep flow folds. Throughout the orogenic operation, granitic rocks of diverse kinds, forms, and positions arise.

OPERATIONS AT HIGHER LEVELS

Although it may be difficult to relate what we can see happened in basements with what goes on at higher levels, in a general way there appear to be at least two patterns of basement deformation—those with linear strikes and those with erratic strikes. As examples of elongated linear granitization and mobility belts, we may cite those of the Laxford zone in the North-West Highlands of Scotland and of Western Tanganyika. The more erratic patterns are given by the Svecofennids of Fennoskandia, the basement of Southern Rhodesia, much of the Nyanzian of Central Africa, and parts of the Canadian Shield. These different styles may underlie different parts of the geosyncline, but a great many variables besides position enter into the matter. It will perhaps be more

rewarding at this stage to ascend into the geosynclines and see what happens there.

The relevant factors include the following. The sedimentary facies of the geosynclinal filling may indicate the nature of the deformation of the main basement below—a matter we have seen to be connected with the appearance of granite. But some basins, or parts of basins, are intruded by granites or are migmatized, some are not; some are regionally metamorphosed, some are not. We have clearly to seek for any relation between the different classes of geosynclinal sediments and the granitic rocks appearing in them. For this purpose, the sediments may be taken in three time groups:

(1) Pre-orogenic sediments, the normal evolutionary series connected with the subsidence of the geosynclinal basin;
(2) Synorogenic sediments, connected with marked instability in or below the basin;
(3) Post-orogenic sediments, worn from the elevated fold-belt.

A survey of the *pre-orogenic sediments* appears to show two rather contrasted styles of metamorphic and granitic phenomena. Over large areas of these rocks, undoubtedly affected by violent orogeny, the metamorphic grade is monotonous and low; only in special environments does it pass beyond the chlorite zone or greenschist facies. The products of this orogenic metamorphism, as Misch (1949) and the writer (Read, 1951; this volume, p. 359) have called it, are the 'universal phyllites' so widespread in many mobile belts. In the domain of orogenic metamorphism, granitic activity is limited to the emplacement of a few diapiric plutons, surrounded by thermal aureoles of the classic Barr-Andlau type, and clearly late arrivals on the scene. Contrasted with this are the great developments of high-grade metamorphic rocks intimately associated with granitization granites and migmatites. Misch (1949) and the writer (Read, 1951; this volume p. 354) and many others consider this association of genetic significance. Indeed, upon it the writer has erected his plutonic rocks division. Wherever we follow rocks into higher metamorphic grades we end up in a granitization core. The field evidence on this matter is good—the writer has listed some of it elsewhere (e.g., Read, 1951, pp. 4–5; this volume, pp. 342–4)—and it holds whether we are magmatists or transformists. But in many examples the core is clearly

granitization granite of syntectonic type, or autochthonous granite of the writer's classification. High-grade metamorphism, granitization, and orogeny are related events. Orogenic metamorphism is an early process in the history of the deformation of the geosynclinal pile; plutonic metamorphism initiated in the depths takes time to extend its domain to the higher levels.

The *synorogenic sediments* are usually in a low metamorphic grade; many of them are almost nonmetamorphic. They are pierced by circumscribed granitic plutons which have made their way in by what is inelegantly called 'plain pushing and shoving'. These plutons are late and have in many places deflected the regional structures, as in the Southern Uplands of Scotland, Devon and Cornwall in England, the area of the Brisbane schists in Queensland, and the Apennines.

Occasionally, however, synorogenic sediments show a higher metamorphic grade, a condition associated with granitization and a special tectonic mobility induced by it. Thus, in the Banff Nappe of North-East Scotland (Read, 1955*b*), a great recumbent fold has a core of migmatitic gneiss; the envelope is made by Upper Dalradian syntectonic graywackes which have been metamorphosed into cordierite-andalusite schists. Here is clearly a case of abnormal mobility brought about by syntectonic migmatization. There are doubtless many other examples; they fit into the general pattern of this thesis.

The *post-orogenic sediments*, those of molasse and Old Red Sandstone type, are never migmatized or granitized and show but little granitic activity. They are intruded by small granite bodies, emplaced by special mechanical contrivances such as cauldron subsidence or graben faulting. These emplacements are clearly high-level operations, appropriate to the setting in which they occur.

It is true that certain granitic bodies occurring in post-orogenic sediments have been interpreted as of granitization origin at that level. This is a matter discussed elsewhere at some length (Read, 1951, pp. 9–10; this volume pp. 349–51): 'the scale of any act of granitization can be judged only from the regional setting of the granite body at the crustal level at which we now see it. Large-scale granitization, applicable throughout a granite mass, must be accompanied by the appropriate apparatus of migmatites and

metamorphites. A small and local demonstration of granitization in the wrong regional setting means nothing so far as the origin at that level of the main body of the associated granite is concerned.' The post-orogenic sediments are clearly the wrong setting for granitization; they are too distant both in time and place from the source of all granite.

From this brief survey, it becomes reasonably manifest that there is a relation between orogeny and granitic phenomena, the stage reached in the orogeny being marked by definite sizes, shapes, and associates of the granites concerned. This relation is, of course, that expressed in the Granite Series, assumed to be valid. We have now therefore to display the Granite Series in all its elegance.

GRANITE SERIES

The writer has always maintained that there are granites and granites, and, for so doing, he has been assailed from both the left and the right. There is really no 'great granite problem'—for, as the Termiers (1953, p. 130) wisely state, it vanishes on the ground. The differences between us arise from gross extrapolation from one granite to all granites; stoping or cauldron subsidence or granitization, perfectly established for one granite, has been applied to all granites. These differences fade away when we apply time, place, and setting to all the granites of one orogeny. We then discover a unity in the diversity of granites, and it is because of this that the writer has ventured to describe the Granite Series as elegant.

The development of ideas on a granite series has already been set out in detail (Read, 1949, pp. 143–51; this volume, pp. 324–37). The French have always held that there were at least two kinds of granite, but as early as 1869 Delesse maintained that 'metamorphic' granites could be driven by orogenic forces into higher levels of the crust there to appear as intrusive. The French duality is retained to a certain extent by the deep granites and intrusive granites of Demay (1942) and the anatectic granites and circumscribed granites of Raguin (1946), but both workers admit intermediate types. Jung and Roques (1938; Roques, 1941) extend the terms into autochthonous, parautochthonous, and

intrusive granites. In all the French work the appearance of granite is related to the orogenic evolution.

Similar ideas on a duality of granites developed in all countries; there was an implied contrast between deep and shallow granites, metamorphic and intrusive granites, migmatitic and magmatic granites, diffuse and circumscribed granites, gneiss massifs and granite stocks, concordant and discordant batholiths, synchronous and subsequent batholiths, Suess and Daly batholiths, Kern-gneisbatholith and granite pluton. This duality fades away as soon as transitions are recognized as significant, the settings are evaluated, and form is related to time. There then appears the unified sequence of plutonic and orogenic events linked up with place that makes the Granite Series. We may proceed to sample the series from a few mobile belts, but before doing so one point must be made clear. We cannot see the progression in the whole Granite Series from initial autochthonous granite to final pluton, any more than the astronomer can see the sequence of nebular configurations from the observation of one nebula. This fact is apparent in the dual form -or time-classifications listed above. In the cores of the fold belts in every continent, older and younger granites have been recognized. There is no need to enumerate such occurrences as they are familiar to everyone. The younger plutons invade the older complexes, but we cannot reasonably derive the plutons from the material of their surrounding complexes; they must come from deeper granitization centres. In the same orogen, however, all the manifestations of granite can be connected. Often in an orogenic belt with a marked one-way tectonic drive, the units of the Granite Series are spread out for separate examination.

The first example of this has been used elsewhere (Read, 1949, pp. 150–1; this volume, pp. 336–7). It is the Hercynian belt of north-western Europe and is outlined in Fig. 18, p. 336. In the Massif Central of France there are great developments of anatectic, deep, or autochthonous granites associated with migmatites and metamorphites; the sedimentary rocks affected are Pre-Cambrian and Lower Palaeozoic. Following these diffuse granites is a genetic series of more clearly intrusive types, till the true intrusive granites penetrate the Visean. In Brittany and Normandy, concordant and discordant granites mostly of Carboniferous age have clearly been guided in their emplacement by Dinantian tectonic axes; the

granites of the deeper structures often have gneissose and felspathized zones associated with them; those in the higher structural positions are more discordant and possess hornfels aureoles. In south-western England, the Carboniferous granites arrived almost dead in their present positions; they are circumscribed plutons showing the granite tectonics of viscous masses. We see the later members of the Granite Series appearing at higher structural and stratigraphical levels as we pass from south to north.

A similar arrangement in place and time is apparent in the Caledonian belt of the British Isles. The plutonic complexes of granitization granites, migmatites, and high-grade metamorphites appear in the Moine and Dalradian of the Grampian Highlands of Scotland and in north-western Ireland, the coherent plutons in the non-metamorphic Lower Palaeozoic of the Scottish Southern Uplands, the English Lake District, and in Leinster, Ireland. The spread is from northwest to southeast, and the plutons are emplaced in the higher structural and stratigraphical positions.

There is no time here unfortunately to present details of more samples of the Granite Series. The writer could have provided illustrations of it from many continents and of many ages, but he must be content with mention of that taking part in the Appalachian *drama*, to use the Termiers' (1953, p. 48–51) excellent term. Here we see the relations between place, time, form, and setting of granitic bodies well displayed. The early Taconic manifestations of the Highlandcroft 'Magma Series' and the reactivated Oliverian complexes of Acadian date contrast, in these respects, with the later Carboniferous ring intrusions and plutons of the White Mountain and associated series.

There is therefore a Granite Series and a relation between granites and granites and orogeny is established.

It has already been emphasized that the progression from one term of the series to another at one level of exposure in the fold-belt is not usually seen. We can, however, illustrate how this progression may occur by consideration of granitic masses that combine the characters of two of the terms. We are here primarily concerned, of course, with mobilization of the autochthonous granite, a topic for which we prepared in the first parts of this essay. It has been shown that granitization leads to mobility in the basement; the autochthonous granites therefore are liable to

become mobile anywhere. The development of ideas on mobilization has been set out in detail elsewhere (e.g., Read, 1943, pp. 70–1; 1944, pp. 54, 71–3, 87–90; 1949, pp. 143–8; 1951, pp. 20–1; see this volume, pp. 57-8, 102, 133-4, 157-61, 324-32, 363-6), and there is no need to resurvey the field now. It will be sufficient to recall a few examples; the writer has already given a fine sample in Kautsky's Scandinavian interpretation (Fig. 22).

Bugge (1943), from his study of the synkinematic granites of the Kongsberg-Bamble region of Southern Norway, concludes that when the granitization granites there were subjected to plastic deformation they moved to give an intrusive contact with the neighbouring rocks. With continued orogenic compression, a magma or something like it was produced which was subsequently intruded into higher levels. Jung and Roques (1938; Roques, 1941) have shown in the Central Massif of France that definitely intrusive granites arise along axes of maximum migmatization and cut the associated autochthonous granitization granites; an example is the Granite du Guéret occurring in the midst of the cordierite-bearing Anatectic Gneiss d'Aubusson. Raguin (1949) comments on the presence of migmatites on the south side of the Quérigut granite in the Pyrenees and their absence on the north, and is tempted to interpret the southern zone as the migmatitic root of the massif and the remainder of the granite as its diapiric derivative, connected with a late Hercynian phase. From the disturbance of ghost structure, Gindy (1953, p. 403) concludes that the migmatites of Lettermackaward in Donegal have been locally mobilized, and the same observer (Gindy, 1954) on similar grounds separates the Aswan granite in Egypt into autochthonous and mobilized portions.

The generalizations concerning mobilization advanced for example by Wegmann (1935*a*; 1936), Backlund (1936), Reynolds (1936*b*), MacGregor and Wilson (1939), and Read (e.g., 1949) appear to be based on as sure a foundation as field-work permits.

The relationship of the different terms of the Granite Series with different stages in the evolution of the fold-belt and the contrast between the early chemical mobility and the later mechanical mobility are too constantly observed to be fortuitous. The writer subscribes to most of the meaning, if not of the wording, of Bucher's (1933) Opinions 25 and 26: 'Granite magmas of orogenic

zones have come into existence as a by-product of orogenic deformation. They rise to the surface differentially as the most mobile of the materials undergoing plastic deformation. Most later intrusives reached the uppermost portion of the crust only as the orogenic pressure was dying down, reaching their final positions by changing places with solid crustal materials through the action of gravity rather than of orogenic pressure.' All granites appear by courtesy of orogeny—maybe the converse is true in some measure.

SOME PROBLEMS OF THE GRANITE SERIES

The Granite Series most fortunately does not solve all the problems connected with granite or with mobile belts. We may briefly consider some of the more obvious.

First, what is the time-span of any one series? It is clear that late members of the series reach up into the post-orogenic sediments which are thereby shown to belong to the same orogeny. The series unites the sedimentary history of the orogen across great unconformities. Thus, pebbles of Moine and Dalradian migmatites, early in the 'Caledonian' Granite Series of the Scottish Highlands, occur in the basal Old Red Sandstone which itself is intruded by later members of the same series in cauldron-subsidence form. Recognition of the Granite Series thus requires a broader view of what an orogeny really is. In the example just cited, the Caledonian orogeny (or, better, the Caledonian drama) can no longer mean what happened in a few hundred thousand years at the close of the Silurian, but must be extended back for hundreds of millions of years to when its Granite Series was initiated. Moreover, it becomes apparent that the later acts of the Caledonian drama had not been performed in the British theatre when the curtain rose on the Hercynian drama in the French theatre to the south; the late members of the Caledonian Granite Series are later than the earlier members of the Hercynian Series.

Backlund (1943, pp. 136–7) has utilized the place of a granite in the Series to determine the limits of an orogeny; a granite with the characteristics of an early term found to be later than another

granite of the same early type must clearly belong to a separate and later orogeny (Read, 1949, p. 149; this volume, p. 334).

The writer has mentioned that younger plutons intrude older granitization granites of the same series. Pitcher (1953*a*; 1953*b*), in the Thorr district of Donegal, Ireland, has shown that a cauldron-subsidence complex of Old Red Sandstone type is there emplaced in granodioritic migmatite; a late and high structural member of the Donegal Granite Series invades an early and deep member. Innumerable examples of comparable occurrences could be cited, especially from the Scottish Highlands, the Massif Central of France, and Scandinavia. During the interval between the formation of the autochthonous granites and the arrival of the latest pluton, the migmatite front has retreated to deeper levels. This is congruent with the whole plutonic and orogenic evolution of the fold belts. But this 'older' granitic migma–magma is not necessarily dead by 'younger' granite times—it may be represented, I suggest, in mobilized form as some members of the dyke swarms that commonly attend the late plutons.

Granitization and granites may take different times to move through the crust. If we are able to date anything about a granite, it is usually its death, and usually not all the members of the same family die on the same day.

Though in this essay most consideration is given to variation of place and time in the Granite Series, there is also a variation in chemical composition. It seems to be a general tendency for the late granites to be more potassic than the earlier terms; most autochthonous and migmatitic granites are soda-dominant types with abundant oligoclase, later granites of the same cycle are potash-dominant with microcline in abundance (Read, 1949, pp. 148–9); this volume, pp. 333–4). Examples of this contrast are innumerable. Wahl (1936), Eskola (1914), and Niggli (1946) have summarized the evidence from Finland whence, too, Simonen (1948) has recently supplied the following figures for the Sveco-fennid Granites.

	Na_2O	K_2O
Synkinematic	3·37	2·80
Late kinematic	2·86	6·02
Post-kinematic	2·85	5·69

In Donegal, Gindy (1953) finds in his Older Granite nearly three times as much plagioclase as microcline, but in the Newer Granite the felspars are equal in amount. Backlund (1943) has assembled the data for the Older and Younger Granites of Fennoskandia: *k* for the Older is 0·42, for the Younger 0·62. If the Granite Series is not just a list of granite forms, then this chemical variation is a matter of great interest, even though it is only a first approximation. A number of suggestions could be made concerning its cause: Backlund (1943) proposes that it results from differential mobilization; magmatists consider it to arise during the normal course (whatever that may be) of crystallization differentiation; other workers suggest that potash is derived from the sediments undergoing granitization; it could be suggested, too, that soda for the early terms likewise comes from the country-rocks. There is a variety of choice. Until the nature of the granitizing fluids is determined, this choice remains open. When detailed geochemical studies of migmatite complexes, such as those begun by Pitcher and Sinha in Donegal, have been made, then perhaps these fluids can be more closely defined and the chemical change in the Granite Series fruitfully discussed.

SPECULATIONS ON A FUNDAMENTAL CAUSE

The writer concludes with some suggestions concerning the prime cause of the phenomena considered here. From what has been said, it seems reasonable to hold that there is a relation between granitization, the appearance of granites, regional metamorphism, and crustal instability. The fundamental cause for operations of this type and on this scale must be sought deep in the crust or below it. For these deep regions, a variety of proposals, most of little weight I am told, is available. That which supposes deep down-buckling of the sial into the sima receives the writer's vote, because with this hypothesis orogenic and granitic phenomena seem to be most compatible.

Such downbuckles, usually as a consequence of subcrustal currents, have been propounded by a number of geologists and geophysicists, Vening Meinesz (1948), Griggs (1939), Umbgrove (1947), Holmes (1944, p. 410), and Fourmarier (1950, p. 1078)

among them. The writer accepts for discussion that these structures are possible and then considers the consequences for granite appearance and orogeny. We agree that the geosynclinal sediments, sial, and sima all enter deeper levels as the downbuckle develops and become liable to change. Frankly the writer does not know what does happen next, and all he proposes to do is to point out that a number of situations favourable to orogeny and the appearance of granite seem likely to develop.

First, mafic magma may be produced from the simatic downbuckle to supply the diabases of the orthogeosynclinal piles and those of the dyke swarms that appear to precede many plutonic cycles. Let us recall the examples already given in the discussion of the use of these dykes in the unravelling of subsequent plutonic events. Wegmann (1948) has suggested that such dyke swarms may indeed prepare the crust for migmatization and the like.

Secondly, in a later stage of the orogeny, the early liquefied sima below the sial downbuckle must crystallize. Many geologists (e.g., Holmes, 1944, p. 485; Umbgrove, 1947, pp. 72, 166; Ramberg, 1948; Fourmarier, 1950, p. 773; Misch, 1949, p. 691) have called upon the sima to supply the granitizing fluids, and the writer (Read, 1951, p. 26; this volume, p. 373) has utilized crystallizing sima as part of the mechanism for the production of the discontinuous continental masses by granitization. The residuals from mafic magma are granitic; indeed, according to Eskola (1950) they may by themselves provide ideal granites. Misch (1949) has considered the source of the granitizing fluids in some detail: the geosynclinal sediments are not rich enough to supply the alkalis required, and the deepest basement shows no signs of having been degranitized, so that the source must be below the exposed depths. Backlund (1953, pp. 89–90, 92, 101) holds that whilst much alkali is supplied by the geosynclinal sediments there must be some influx of NaSi from below. Misch (1949, p. 691) has called attention to the circumstance that granitization appears to be confined to eugeosynclinal belts, that is to geosynclinal piles that have mafic materials in them. If this is indeed a fact, then it is agreeable to the suggestions just made concerning the consequences of the liquefaction and subsequent crystallization of sima at the base of the downbuckle. The writer holds that granites and basalts are different things, but, in their

ultimate source, they may thus be closely and genetically connected (cf. Backlund, 1953, p. 101).

Once granitization has begun in the sial, it will proceed with the evolution of the orogenic belt along the lines suggested in the foregoing pages. Chemical mobility is followed by mechanical mobility, the basement becomes reactivated, anatexis and related processes contribute their quota, and the terms of the Granite Series appear in orderly progression.

This essay, being largely concerned with field-observations, may not always have dealt with what are sometimes called facts. But that in no way vitiates its purpose which was to promote discussion as a spur to further effort. It has underlined the necessity of time and space studies in the plutonic rocks of the mobile belts. In conclusion, the writer desires to thank his fellow research workers at Imperial College, London; they, young in years and fertile in ideas, will have time to demolish, or not, whatever is said here.

BIBLIOGRAPHY

ADAMS, F. D., 1912, The origin of the deep-seated metamorphism of the pre-Cambrian crystalline schists, *C.R. XI Congr. géol. intern.* (*Stockholm* 1910) **1**, 572.

1930, The transfusion of matter from one solid to another under the influence of heat—a new factor in the process of metamorphism, *Canadian Journ. Research* **2**, 153.

AND A. E. BARLOW, 1910, The geology of the Haliburton and Bancroft areas, *Canada Geol. Survey, Mem.* **6**.

AMBROSE, J. W., 1936, Progressive kinetic metamorphism of the Missi series, near Flinflon, Manitoba, *Am. Journ. Sci.*, 5 ser., **32**, 257.

ANDERSON, A. L., 1934, Contact phenomena associated with the Cassia batholith, Idaho, *Journ. Geol.* **52**, 376.

ANDERSON, G. H., 1937, Granitization, albitization and related phenomena in the Northern Inyo Range of California-Nevada, *Bull. Geol. Soc. Am.* **48**, 1.

BACKLUND, H. G., 1918, Petrogenetische Studien an Taimyrgesteinen, *Geol. Fören. Stockholm Förh.* **40**, 101.

1936, Der 'Magmaaufsteig' in Faltengebirgen, *Bull. Com. géol. Finlande*, no. 115, 293.

1937, Die Umgrenzung der Svecofenniden, *Bull. Geol. Inst. Upsala* **27**, 219.

1938*a*, The Rapakiwi puzzle, a reply, *Geol. Fören. Stockholm Förh.* **60**, 105.

1938*b*, Zur Granitisationstheorie; eine Verdeutlichung, *Geol. Fören. Stockholm. Förh.* **60**, 177.

1938*c*, The problems of the Rapakivi granites, *Journ. Geol.* **46**, 339.

1943, Einblicke in das geologische Geschehen des Präkambriums, *Geol. Rundsch.* **34**, 79

1946, The granitization problem, *Geol. Mag.* **83**, 105.

1953, The granitization problem, *Instituto 'Lucas Mallada', Estudios Geologicos* **9**, 71.

BAILEY, E. B., 1923, The metamorphism of the south-west Highlands, *Geol. Mag.* **60**, 317.

1937, American gleanings: 1936 *Trans. Geol. Soc. Glasgow* **20**, 1.

AND D. TAIT, 1921, *Edinburgh's Place in Scientific Progress.*

BALK, R. AND T. F. W. BARTH, 1936, Structural and petrologic studies in Dutchess County, New York. Part I, Geologic structure of sedimentary rocks (BALK), *Bull. Geol. Soc. Am.* **47**, 685, Part II, Petrology and metamorphism of the Palaeozoic Rocks (BARTH), *ibid.*, 775.

BARBOUR, G. B., 1930, Origin of the Bedford augen-gneiss, *Am. Journ. Sci.*, 5 ser., **19**, 351.

BARRELL, J., 1921, Relation of subjacent igneous intrusion to regional metamorphism, *Am. Journ. Sci.*, 5 ser., **1**, 185.

BARROIS, C., 1884, Mémoire sur le granite de Rostrenon, *Ann. Soc. géol. Nord* **12**, 14.

BARROW, G., 1892, On certain gneisses with round-grained oligoclase and their relation to pegmatites, *Geol. Mag.* Dec. iii, **9**, 64.

1893, On an intrusion of muscovite-biotite-gneiss in the south-eastern Highlands of Scotland, and its accompanying metamorphism, *Quart. Journ. Geol. Soc.* **49**, 330.

1896, *In* Annual report of the Geological Survey for the year ending December 31, 1895, 23.

1898, *In* Summary of progress of the Geological Survey of the United Kingdom for 1897, *Mem. Geol. Surv.*, 51.

1905, The geology of the country round Blair Athol, Pitlochry and Aberfeldy, *Mem. Geol. Surv. Scotland.*

1912*a*, The geology of the districts of Braemar, Ballater and Glen Clova, *Mem. Geol. Surv. Scotland.*

1912*b*, On the geology of Lower Deeside and the southern Highland Border, *Proc. Geol. Assoc.* **23**, 274.

BARTH, T. F. W., 1936, The large Pre-Cambrian intrusive bodies in the southern part of Norway, *C.R. XVI Congr. géol. intern.* (Washington 1936) 1, 297.

1938, Progressive metamorphism of the sparagmite rocks of southern Norway, *Norsk Geol. Tidskr.* **18**, 54.

C. W. CORRENS AND P. ESKOLA, 1939, *Die Entstehung der Gesteine*, Berlin.

BASTIN, E. S. AND OTHERS, 1931, Criteria of age relations of minerals with special reference to polished sections of ores, *Econ. Geol.* **26**, 561.

BEAUMONT, E. DE, 1847, Note sur les émanations volcaniques et métallifères, *Bull. Soc. géol. Fr.*, 2 sér., **4**, 1247.

BECKE, F. 1882, Die Gneisformation des niederösterreichischen Waldviertels, *Tscherm. Min. Pet. Mitt.* **4**, 189.

1892, Vorläufiger Bericht über den geologischen Bau und die kristallinen Schiefer des Hohen Gesenkes (Altvatergebirge), *Sitzber. k. Akad. Wiss. Wien, Mat.-Nat. Kl.* **51**, 294.

1904, Über Mineralbestand und Struktur der kristallinischen Schiefer, *C.R. IX Congr. géol. intern.* (*Vienne* 1903), 553.

1909, Über Diaphthorite, *Tscherm. Min. Pet. Mitt.* **28**, 369.

1912, Über das Grundgebirge im niederösterreichischen Waldviertel, *C.R. XI Congr. géol. intern.* (*Stockholm* 1910), 617.

1913, Über Mineralbestand und Struktur der kristallinischen Schiefer, *Denkschr. k. Akad. Wiss. Wien. Mat.-Nat. Kl.* **75**, 1.

1921, Zur Facies-Klassification der metamorphen Gesteine, *Min. Pet. Mitt.* **35**, 215.

1924, Struktur und Kluftung, *Fortschr. Min. Krist. Petr.* **9**, 185.

BERG, G., 1910, Die Entstehung der Orthogneise, *Zeit. d. deutsch. geol. Gesellsch.* **62**, 344.

1912, Die krystallinen Schiefer des östlichen Riesengebirges, *Abh. k.-preuss. geol. Landesanst.* N.S. **68**, 1.

1913, Granitstöcke und Gneismassive, *Geol. Rundsch.* **4**, 225.

1923, Der Granit des Riesengebirges und seine Ganggesteine, *Abh. preuss. geol. Landesanst.* N.S. **94**, 1.

BILLINGS, M. P., 1928, The petrology of the North Conway Quadrangle in the White Mountains of New Hampshire, *Proc. Am. Acad. Arts and Sci.* **63**, 67.

1937, Regional metamorphism of the Littleton-Moosilauke area, New Hampshire, *Bull. Geol. Soc. Am.* **48**, 463.

1941, Structure and metamorphism in the Mount Washington area, New Hampshire, *Bull. Geol. Soc. Am.* **52**, 863.

1945, Mechanics of igneous intrusion in New Hampshire *Am. Journ. Sci.*, 243A (Daly volume), 40.

BOULE, M., 1900, Géologie des environs d'Aurillac et observations nouvelles sur le Cantal, *Bull. Carte géol. France* 11, No. 76.

BOWEN, N. L., 1910, Diabase and granophyre of the Gowganda Lake District, *Journ. Geol.* **18**, 658.

1915, Crystallization-differentiation in silicate liquids, *Am. Journ. Sci.*, 4 ser., **39**, 175.

1928, *The Evolution of the Igneous Rocks.*

1947, Magmas, *Bull. Geol. Soc. Am.* **58**, 263.

BOWES, D. R., 1954, The transformation of tillite by migmatization at Mount Fitton, South Australia, *Quart. Journ. Geol. Soc.* **109**, 455.

BRAMMALL, A., 1933, Syntexis and differentiation, *Geol. Mag.* **70**, 97.

AND H. F. HARWOOD, 1932, The Dartmoor granites: their genetic relationships, *Quart. Journ. Geol. Soc.* **88**, 171.

BROWNE, W. R., 1931, Notes on bathyliths and some of their implications, *Journ. Proc. Roy. Soc. N.S.W.* **65**, 112.

BUBNOFF, S. von, 1921, Die hercynischen Brüche im Schwarzwald, ihre Beziehung zur carbonischen Faltung und ihre Posthumität, *N. J. f. Min, Beil.-Bd.* **45**, 1.

1922, Tektonik und Intrusionsmechanismus im kristallinen Odenwald, *Abh. preuss. geol. Landesanst.* N.F. **89**, 19.

1930, *Geologie von Europa*, Bd. ii, Teil 1, Berlin.

BUCHER, W. H., 1933, *The Deformation of the Earth's Crust*, Princeton, N.J.

BUGGE, J. A. W., 1943, Geological and petrographical investigations in the Kongsberg-Bamble formation, *Norges Geol. Undersök.*, no. 160.

1946. The geological importance of diffusion in the solid state, *Skrifter Norske Videnskaps-Akad. Oslo I Mat.-Nat. Kl.*, 1945, no. 13.

CHAPMAN, C. A., 1939, The geology of the Mascoma Quadrangle, New Hampshire, *Bull. Geol. Soc. Am.* **50**, 127.

CHENG, YU-CHI, 1942, A hornblendic complex, including appinitic types, in the migmatite area of North Sutherland, Scotland, *Proc. Geol. Assoc.* **53**, 67.

1944, The migmatite area around Bettyhill, Sutherland, *Quart. Journ. Geol. Soc.* **99** (for 1943) 107.

CLOOS, E., 1936, Der Sierra Nevada Pluton in Californien, *N. J. f. Min. Geol. Palaeon. Beil.-Bd.* **76**, *Abt. B*, 355.

AND C. H. BROEDEL, 1940, Geologic map of Howard County: Scale 1 : 62,500, *Maryland Geol. Surv.*

AND ANNA HIETANEN, 1941, Geology of the 'Martic Overthrust' and Glenarm series in Pennsylvania and Maryland, *Geol. Soc. Am. Special Papers*, no. 35.

CLOOS, H. AND A. RITTMANN, 1939, Zur Einteilung und Benennung der Plutone, *Geol. Rundsch.* **30**, 600.

CLOUGH, C. T., 1897, *in* W. GUNN AND OTHERS, The geology of Cowal, *Mem. Geol. Surv. Scotland.*

COLE, G. A. J., 1894, On derived crystals in the basaltic andesite of Glasdrumman Port, Co. Down, *Sci. Trans. Roy. Dublin Soc.*, 2 ser., **5**, 239.

1902, Composite gneisses in Boylagh, West Donegal, *Proc. Roy. Irish Acad.* **24**, B, 203.

COLEMAN, A. C., 1912, Metamorphism in the Pre-Cambrian of Northern Ontario, *C.R. XI Congr. géol. intern.* (*Stockholm* 1910), 615.

COLLINS, W. H., 1936, The origin of granitic rocks, *Rep. XVI Intern. Geol. Congr.* (*Washington* 1933), I, 271.

COLONY, R. J., 1928, Ore-mineral sequence, *The Laboratory Investigation of ores: a Symposium* (E. E. FAIRBANKS, Editor), New York, 132–46.

CORIN, F., 1930, Le métamorphisme de l'Ardenne, *Ann. Soc. géol. Belg.* **54**, 110.

1932, Coup d'œil sur la zone métamorphique du Paliseul, *Bull. Soc. Belge Géol.* **41**, 340.

CORNELIUS, H. P., 1921, Über einige Probleme der pennischen Zone der Westalpen, *Geol. Rundsch.* **11**, 289.

1935, Zur Deutung gefüllte Feldspäte, *Schweiz. Min. Pet. Mitt.* **15**, 4.

CUNNINGHAM-CRAIG, E. H., 1904, Metamorphism in the Loch Lomond District, *Quart. Journ. Geol. Soc.* **60**, 10.

CURRIER, L. W., 1947, Granitization and its significance as a regional metamorphic process in New England, *Journ. Wash. Acad. Sci.* **37**, 75.

DALY, R. A., 1915, A geological reconnaissance between Golden and Kamloops, B.C., *Canada Geol. Surv. Mem.* **68**.

1917, Metamorphism and its phases, *Bull. Geol. Soc. Am.* **28**, 375.

1933, *Igneous Rocks and the Depths of the Earth.*

DARWIN, C., 1846, *Geological Observations on South America.*

DAVIDSON, C. F., 1943, The Archean rocks of the Rodil district, South Harris, Outer Hebrides, *Trans. Roy. Soc. Edin.* **61**, 71.

DAWSON, G. M., 1901, *Bull. Geol. Soc. Am.* **12**, 64.

DELESSE, A., 1858, *Études sur le Métamorphisme des Roches.*

1869, *Études sur le Métamorphisme des Roches.*

DeLURY, J. S., 1941, Correlation of schistosity and tectonic theory, *Am. Journ. Sci.* **239**, 57.

DEMAY, A., 1942, Microtectonique et tectonique profonde; cristallisations et injections magmatiques syntectoniques, *Mém. Serv. Carte géol. France.*

1946*a*, Explication de la carte géologique des terrains pré-cambriens et paléozoïques du Massif Central, *Mém. Serv. Carte géol. France.*

1946*b*, Notice sur les travaux scientifiques de M. ANDRE DEMAY, Paris.

1946*c*, Sur la nappe antéstéphanienne de la Margeride dans la région médiane du Massif Central, *C.R. Acad. Sci. Paris* **222**, 1119.

DEVILLE, H. ST.-C., 1841, *C.R. Acad. Sci. Paris.* **52**, 1264.

1878, *Coup d'œil historique sur la Géologie et sur les Travaux d'Élie de Beaumont*, Paris.

DRESCHER, F. K., 1926, Über granito-dioritische Mischgesteine des Friedeberger Intrusivmasse, *N. J. f. Min. Beil.-Bd.* **54**A, 243.

DUNN, J. A., 1942, Granite and magmation and metamorphism, *Econ. Geol.* **37**, 231.

DUROCHER, J., 1847, Note sur une espèce de granite provenant de la Normandie et de la Bretagne, *Bull. Soc. géol. Fr.* 2 ser., **4**, 140.

1857, *Ann. des Mines* **11**, 217.

DU REITZ, T., 1938, The injection metamorphism of the Muruhatten region, *Sver. Geol. Undersök. Arsb.* **32**, no. 6.

DU TOIT, A. L., 1937, *Our Wandering Continents*, Edinburgh.

EBERT, H., 1935, Hornfelsbildungen und Anatexis im Lausitzer Massiv, *Zeit. d. deutsch. geol. Gesellsch.* **87**, 129.

ECKERMANN, H. von, 1936, The Loos-Hamra Region, *Geol. Fören. Stockholm Förh.* **58**, 129.

1937, The genesis of the Rapakivi granites, *ibid.* **59**, 503.
1938, The Rapakivi facts, an answer to a reply, *ibid.* **60**, 113.
EDELMAN, N., 1949, Structural history of the eastern part of the Gullkrona Basin, SW. Finland, *Bull. Comm. géol. Finlande*, No. 148
EDWARDS, A. B., 1947, Textures of the ore minerals and their significance, *Austr. Inst. Min. Met.*
EITEL, W., 1941, *Physikalische Chemie der Silikate*, Leipzig.
ELLES, G. L. & C. E. TILLEY, 1930, Metamorphism in relation to structure in the Scottish Highlands, *Trans. Roy. Soc. Edin.* **56**, 621.
ERDMANNSDÖRFFER, O. H., 1911, Der Eckergneis im Harz, *Jahrb. preuss. geol. Landesanst. für* 1911, 324.
1928, Über Disthen-Andalusitparagenes, *Sitzungsber. Heidelberg. Akad. Wiss. Mat.-Nat. Kl.*, no. 16.
1936, Neuere Aıbeiten über Metamorphismus und seine Grenzgebiete *Fortsch. Min. Krist. Pet.* **20**, 143.
ESKOLA, P. E., 1914, On the petrology of the Orijärvi region in south-western Finland, *Bull. Com. géol. Finlande*, no. 40.
1915, On the relation between the chemical and mineralogical composition in metamorphic rocks of the Orijärvi region, *ibid.* no. 44.
1922, The mineral facies of rocks, *Norsk Geol. Tidsskr.* **6**, 143.
1925, On the petrology of eastern Fennoskandia: I. The mineral development of basic rocks in the Karelian formations, *Fennia* **45**, No. 19.
1927, Petrographische Charakterstik der kristallinen Gesteine von Finnland, *Fortsch. Min. Krist. Pet.* **11**, 57.
1932*a*, Conditions during the earliest geological times as indicated by the Archean rocks, *Ann. Acad. Sci. Fennicae*, ser. A, **36**, no. 4.
1932*b*, On the principles of metamorphic differentiation, *Bull. Com. géol. Finlande*, no. 97 (*C. R. Soc. géol. Finlande*, 5), 68.
1932*c*, On the origin of granitic magmas, *Min. Pet. Mitt.* **42**, 455.
1933, On the differential anatexis of rocks, C.*R. Soc. géol. Finland*, no. 7, 12.
1934, A note on diffusion and reactions in solids *Bull. Com. géol. Finlande*, no. 104 (*C. R. Soc. géol. Finlande* 8), 144.
1939, Die metamorphen Gesteine, *in* BARTH, CORRENS and ESKOLA, 1939, 263–407.
1949, The problem of mantled gneiss domes, *Quart. Journ. Geol. Soc.* **104**, 461.
1950, The nature of metasomatism in the processes of granitization, *Rep. XVIII Int. Geol. Congr.* (*Great Britain* 1948), pt. 3, 5.
1951, Around Pitkaranta, *Ann. Acad. Sci. Fnenicae* **3**, *Geol. Geogr. Ser. A.*27, 1.
1952, A discussion of domes and granites and ores, *C.R. Soc. géol. Finlande*, no. 25, 125.
EXNER, C., 1947, Das Gneisproblem in den östlichen Hohen Tauern, *Wien Min.-Gesell. Mitt.* no. 10, **1**, 82, *Tscherm. Min. Pet. Mitt.* **1**, 82.
1948, Tektonik, Feldspatausbildungen und deren gegenseitige Beziehungen in den östlichen Hohen Tauern, *Tscherm. Min. Pet. Mitt.* **1**, 197.
1949, Die Feldspatholoblasten des alpidischen Granitisationshofes im Radhausberg-Unterbaustollen bei Badgastein, *Öst. Akad. Wiss. Ang. Mat.-Nat. Kl.* no. 13, 267.
FAIRBAIRN, H. W., 1943, Packing in ionic minerals, *Bull. Geol. Soc. Am.* **54**, 1305.
FALLOT, P., 1939, Élie de Beaumont et l'évolution des sciences géologiques au Collège de France, *Ann. des Mines*, 13 sér., **15**, 75.

FENNER, C. N., 1914, The mode of formation of certain gneisses in the Highlands of New Jersey, *Journ. Geol.* **22**, 594.

1929, The crystallization of basalts, *Am. Journ. Sci.*, 5 ser., **18**, 225.

1931, The residual liquids of crystallizing magmas, *Mineral. Mag.*, 22, 539.

1933, Pneumatolytic processes in the formation of minerals and ores, *Ore Deposits of the Western States*, 58.

FERNANDO, L. J. D., 1941, Petrology of certain felspathized rocks from Herma Ness, Unst, Shetland Islands, *Proc. Geol. Assoc.* **52**, 110.

FLETT, J. S., 1912, *In* The geology of Ben Wyvis, &c., *Mem. Geol. Surv. Scotland.*

FOURMARIER, P., 1950, *Principes de Géologie*, Paris-Liège.

FOURNET, J., 1847*a*, Résultats sommaires d'une exploration des Vosges, *Bull. Soc. géol. Fr.* 2 sér., **4**, 220.

1847*b*, *Die Metamorphose der Gesteine nachgewiesen in den westlichen Alpen*, Freiberg (translated by W. VOGELSANG).

GEIJER, P., 1916, On the intrusion mechanism of the Archean granites of central Sweden, *Bull. Geol. Inst. Univ. Upsala* **15**, 47.

GILLULY, J., 1937, The water content of magmas, *Am. Journ. Sci.*, 5 ser., **33**, 430.

GINDY, A. R., 1953, The plutonic history of the district around Trawenagh Bay, Co. Donegal, *Quart. Journ. Geol. Soc.* **108**, 377.

1954, The plutonic history of the Aswan area, Egypt, *Geol. Mag.* **79**, 484.

GOGUEL, J., 1943, Introduction à l'étude mécanique des déformations de l'écorce terrestre, *Mém. Serv. Carte géol. France.*

GOLDSCHMIDT, V. M., 1912*a*, Geologisch-petrographische Studien im Hochgebirge des südlichen Norwegens. I. Ein kambrisches Konglomerat von Finse und dessen Metamorphose, *Videnskaps. Skrifter I. Mat.-Nat. Kl.* 1912, no. 18.

1912*b*, Geologisch-petrographische Studien im Hochgebirge des südlichen Norwegens. II. Die Kaledonische Deformation der südnorwegischen Urgebirgstafel, *ibid.*, no. 19.

1915, Geologisch-petrographische Studien im Hochgebirge des südlichen Norwegens. III. Die Kalksilikatgneise und Kalksilikatglimmerschiefer des Trondhjem-Gebiets, *ibid.*, 1915, no. 10.

1916, Geologisch-petrographische Studien im Hochgebirge des südlichen Norwegens. IV. *ibid.*, 1916, no. 2.

1921, Geologisch-petrographische Studien im Hochgebirge des südlichen Norwegens. V. Die Injektionsmetamorphose im Stavanger-Gebiete, *ibid.*, 1920, no. 10.

1922, On the metasomatic processes in silicate rocks, *Econ. Geol.* **17**, 105.

GORANSON, R. W., 1931, The solubility of water in granite magmas, *Am. Journ. Sci.* **22**, 481.

GRANTHAM, D. R., 1928, The petrology of the Shap granite, *Proc. Geol. Assoc.* **39**, 299.

GRATON, L. C., 1940, Nature of the ore-forming fluid, *Econ. Geol.* **35**, 205.

1945, Conjectures regarding volcanic heat, *Am. Journ. Sci.* **243A** (Daly volume) 135.

GREEN, A. H., 1882, *Physical Geology.*

GREENLY, E., 1903, The diffusion of granite into crystalline schists, *Geol. Mag.* Dec. 4, **10**, 207.

1926, Orders of validity in geological theory, *Geol. Mag.* **63**, 174.

GREIG, J. W., E. S. SHEPHERD and H. E. MERWIN, 1931, *Annual Rep. Director Geophysical Lab. Washington*, 77.

GRIGGS, D., 1939, A theory of mountain building, *Am. Journ. Sci.* **237**, 611.
GROUT, F. F., 1926, The use of calculations in petrology, *Journ. Geol.* **34**, 549.
1932, *Petrography and Petrology*.
1937, Criteria of origin of inclusions in plutonic rocks, *Bull. Geol. Soc. Am.* **48**, 1521.
1941, The formation of igneous-looking rocks by metasomatism, *ibid.* **52**, 1525.
GRUBENMANN, U., 1910, *Die kristallinen Schiefer*, 2 edition, Berlin.
AND P. NIGGLI, 1924, *Die Gesteinsmetamorphose*, I. *Allgemeiner Teil*, Berlin.
HALL, A. L., 1932, The Bushveld Igneous Complex, *Geol. Surv. S. Africa*, mem. 28
HARKER, A., 1904, Tertiary Igneous Rocks of Skye, *Mem. Geol. Surv. Scotland.*
1909, *The Natural History of the Igneous Rocks.*
1919*a*, *Petrology for Students*, 5 edition.
1919*b*, (Metamorphism in Rock-masses) Presidential Address, *Quart. Journ. Geol. Soc.* **74** (for 1918), lxiii.
1932, *Metamorphism*, London (2 edition, 1939).
AND J. E. MARR, 1891, The Shap granite and its associated igneous and metamorphic rocks, *Quart. Journ. Geol. Soc.* **47**, 286.
HARVEY, R. D., 1931, The geometrical pattern of contacts in determinative paragenesis, *Econ. Geol.* **26**, 764.
HATCH, F. H. and A. K. WELLS, 1937, *The Petrology of the Igneous Rocks.*
HAUGHTON, S., 1862, Annual address delivered before the Geological Society of Dublin, Feb. 12, 1.
HEIM, A., 1878, *Untersuchungen über den Mechanismus der Gebirgsbildung*, Basel.
HESS, H. H., 1933, Hydrothermal metamorphism of an ultrabasic intrusive at Schuyler, Virginia, *Am. Journ. Sci.*, 5 ser., **26**, 377.
HOLMES, A., 1920, *Nomenclature of Petrology*, London.
1931, The problem of the association of acid and basic rocks in central complexes, *Geol. Mag.* **68**, 241.
1932, The origin of igneous rocks, *Geol. Mag.* **69**, 543.
1936*a*, Transfusion of quartz xenoliths in alkali basic and ultrabasic lavas, south-west Uganda, *Mineral. Mag.* **24**, 408.
1936*b*, The idea of contrasted differentiation, *Geol. Mag.* **73**, 228.
1944, *Principles of Physical Geology*, Edinburgh.
1945, Natural history of granite, *Nature* **155**, 412.
AND H. F. HARWOOD, 1937, The volcanic area of Bufumbira; Part II, The petrology of the volcanic field of Bufumbira, south-west Uganda, *Mem. Geol. Surv. Uganda*, no. 3.
HOLMQUIST, P. J., 1901, Rapakiwistruktur och granitstruktur, *Geol. Fören. Stockholm Förh.* **23**, 150.
1907, Adergneisbildung und magmatische Assimilation im Grundgebirge Schwedens, *ibid.* **29**, 313.
1910, Die Hochgebirgsbildungen am Tornetrask in Lappland, *ibid.* **32**, 913.
1920, Om pegmatit-palingenes och ptygmatisk veckning, *ibid.* **42**, 191.
1921, Typen und Nomenklatur der Adergesteine, *ibid.* **43**, 612.
HOLSER, W. T., 1947, Metasomatic processes, *Econ. Geol.* **42**, 384.
HOLTEDAHL, O., 1952*a*, The structural history of Norway and its relation to Great Britain, *Quart. Journ. Geol. Soc.* **108**, 65.
1952*b*, Deep-seated crustal deformations in a north-western part of the Caledonides of Southern Norway, *Rep. XVIII Intern. Geol. Congr.* (*Great Britain* 1948), pt. 13, 135.
HORNE, J. and E. GREENLY, 1896, On foliated granites and their relations to the crystalline schists in eastern Sutherland, *Quart. Journ. Geol. Soc.* **52**, 633.

HUTTON, C. O., 1940, Metamorphism in the Lake Wakatipu region, Western Otago, New Zealand, *N.Z. Dep. Sci. Ind. Res. Geol. Mem.*, no. 5.

AND F. J. TURNER, 1936, Metamorphic zones in north-west Otago, *Trans. Roy. Soc. N.Z.* **65**, 405.

HUTTON, J. ,1795, *The Theory of the Earth*, Edinburgh.

IDDINGS, J. P., 1909, *Igneous Rocks.*

INGERSON, E., 1938, Albite trends in some rocks of the Piedmont, *Am. Journ. Sci.*, 5 ser., **35**, 127.

JAMOTTE, A., 1940, Sur un principe de mobilité de l'émanation ou de l'intrusion, *Com. Spéc. du Katanga.*

JOHANNSEN, A., 1931, *Descriptive Petrology of the Igneous Rocks.*

JOHNSON, C. H., 1947, Igneous metamorphism in the Orofino region, Idaho, *Journ. Geol.* **55**, 490.

JUDD, J. W., 1889, On statical and dynamical metamorphism, *Geol. Mag.*, 243.

JUNG, J., 1928, Contribution à la géologie des Vosges hercyniennes d'Alsace, *Mém. Serv. Carte géol. Alsace Lorraine*, no. 2.

1936, Sur l'existence et le rôle des migmatites dans le terrain cristallophyllien des Vosges, *C.R. Soc. géol. France*, 1936, 285.

AND M. ROQUES, 1938, Les schistes cristallins du Massif Central, *Bull. Serv. Carte géol. France*, no. 197 (xxxix), 120.

KARPOFF, R., 1946, Sur le présence de disthène dans une pegmatite à la frontière algéro-soudanaise, *C.R. Acad. Sci. Paris* **223**, 1154.

KAUTSKY, G., 1946, Neue Gesichtspunkte zu einigen Nordskandinavischen Gebirgsproblemen, *Geol. Fören. Stockholm Förh.* **68**, 589.

1947, Neue Gesichtspunkte zu einigen Nordskandinavischen Gebirgsproblemen, *ibid.* **69**, 108.

KENNEDY, W. Q., 1948, On the significance of thermal structure in the Scottish Highlands, *Geol. Mag.* **85**, 229.

AND E. M. ANDERSON, 1938, Crustal layers and the origin of magmas, *Bull. Volc.*, ser. ii, **3**, 24.

KLÜPFEL, W., 1941, Ueber die Altvulkane und die Neuvulkane und ihre Abstammung, *Zentralbl. Min. Abt. B*, 230, 249, 281, 313.

KNOPF, A., 1948, The geosynclinal theory, *Bull. Geol. Soc. Am.* **59**, 649.

KNOPF, E. B., 1931, Retrogressive metamorphism and phyllonitization, *Am. Journ. Sci.*, 5 ser., **21**, 1.

1935, Recognition of overthrusts in metamorphic terranes *Am. Journ. Sci.*, 5 ser., **30**, 198.

1938, *In* Final Report of the Committee on Structural Petrology, 1937, *National Research Council, Washington.*

KOENIGSBERGER, J., 1912*a*, Die kristallinen Schiefer der zentralschweizerischen Massive und Versuch einer Einteilung der kristallinen Schiefer, *C.R. XI Congr. géol. intern.* (Stockholm 1910), 639.

1912*b*, Über Gneisbildung und Aufschmelzungszonen der Erdkruste in Europa, *Geol. Rundsch.* **3**, 297.

KRANCK, E. H., 1935, On the crystalline complex of Liverpool Land, *Medd. om Grönland* **95**, no. 7.

1937, Beiträge zur Kenntnis der Svecofenniden in Finnland: IV. Über Intrusion und Tektonik im Kustengebiete zwischen Helsingfors und Porkala *Bull. Com. géol. Finlande*, no. 119 (*C.R. Soc. géol. Finlande* **10**), 69.

1953, Interpretation of Gneiss Structures with special reference to Baffin Island, *Proc. Geol. Assoc. Canada* **6**, 59.

KRIGE, L. J., 1916, Petrographische Untersuchungen im Val Piora und Umgebung, *Eclog. Geol. Helv.*, **14**, 519.

LACROIX, A., 1898-9, Le granite des Pyrénées et ses phénomènes de contact: Premier Mémoire, *Bull. Serv. Carte. géol. France*, no. 64, X.

1899–1900, Le granite des Pyrénées et ses phénomènes de contact: Deuxième Mémoire, *ibid.*, no. 71, XI.

LAPADU-HARGUES, P., 1945, Sur l'existence et la nature de l'apport chimique dans certaines séries cristallophylliennes, *Bull. Soc. géol. France*, 5 sér., **15**, 255.

1948, Les massifs de la Margeride et du Mont Lozère et leurs bordures, *Bull. Serv. Carte. géol. France*, no. 222 (XLVI, 1947).

LAPPARENT, J. DE, 1923, *Leçons de Pétrographie.*

1935, Rythmes du métamorphisme dans les Highlands, *Bull. Soc. géol. France*, 5 sér., **5**, 281.

1941, Logique des minéraux du granite, *Rev. Scientifique*, 285.

LARSEN, E. S. and P. W. BRIDGMAN, 1938, Shearing experiments on some selected mineral combinations, *Am. Journ. Sci.* **36**, 81.

LEHMANN, J., 1884, *Untersuchungen über die Entstehung der altkristallinen Schiefergesteine im sächsische Granulitgebirge*, Bonn.

LEPSIUS, R., 1893, *Geologie von Attika*, 191.

1912, Über die wesentlichen Unterschiede zwischen diskordanten und konkordanten Granitstocken, usw., *Geol. Rundsch.* **3**, 1.

LINDGREN, W., 1933, *Mineral Deposits*, 4 edition, 166–90.

LOCKE, A., 1941, Granite and ore, *Econ. Geol.* **36**, 448.

P. BILLINGSLEY AND E. B. MAYO, 1940, Sierra Nevada Tectonic pattern, *Bull. Geol. Soc. Am.* **51**, 513.

LOEWINSON-LESSING, F., 1911, The fundamental problems of petrogenesis, or the origin of the igneous rocks, *Geol. Mag.* Dec. 5, **8**, 248.

1936, A contribution to the mechanics of intrusion, *Rep. XVI Intern. Geol. Congr.*, (*U.S.A.* 1933) I, 333.

LONGWELL, C. R., 1945, The mechanics of orogeny, *Am. Journ. Sci.* **243A** (Daly volume), 417.

LONSDALE, KATHLEEN, 1947, The structure of real crystals, *Science Progress* **35**, 1.

LOSSEN, K. A., 1875, *Zeitschr. deutsch. geol. Gesell.* **27**, 970.

LYELL, C., 1830, *Principles of Geology*, 1 edition, 1.

1833, *Principles of Geology*, 1 edition, 3.

1835, *Principles of Geology*, 4 edition.

1838, *Elements of Geology*, 1 edition.

1871, *Student's Elements of Geology.*

1875, *Principles of Geology*, 12 edition.

MACGREGOR, M. AND G. WILSON, 1939, On granitization and associated processes, *Geol. Mag.* **76**, 193.

MCCALLIEN, W. J., 1934, Metamorphic diffusion, *Bull. Com. géol. Finlande*, no. 104 (*C.R. Soc. géol. Finlande* **8**), 11.

MAGNUSSON, N. H., 1938, Die Granitisationstheorie und deren Anwendung für Svionische Granite und Gneise Mittelschwedens, *Geol. Fören. Stockholm Förh.* **59**, (for 1937), 525.

MAYO, E. B., 1941, Deformation in the interval Mt. Lyell–Mt. Whitney, California, *Bull. Geol. Soc. Am.* **52**, 1001.

MEINESZ, F. A. VENING, 1948, Major tectonic phenomena and the hypothesis of convection currents in the earth, *Quart. Journ. Geol. Soc.* **103** (for 1947), 191.

MICHEL-LÉVY, A., 1879, Note sur la formation gneissique du Morvan, &c., *Bull. Soc. géol. France*, 3 sér., **7**, 857.

1887a, Sur l'origine des terrains cristallins primitifs, *ibid.*, 3 sér., **16**, 102.

1887b, Sur les roches éruptives et cristallines des montagnes du Lyonnais, *ibid.*, 3 sér., **16**, 216.

1889, *Structure et Classification des Roches Eruptives.*

1893–4, Contributions à l'étude du granite de Flamanville et des granites français en général, *Bull. Serv. Carte. géol. France*, V.

1896, Sur quelques particularités du porphyre bleu d'Esterel, &c., *Bull. Soc. géol. France*, 3 sér., **24**, 123.

1897, Note sur la classification des magmas des roches eruptives, *ibid.*, 3 sér., **25**, 326.

MILCH, L., 1894, Beiträge zur Lehre von der Regionalmetamorphose, *Neues Jahrb. f. Min. Geol. u. Pal. Beil.-Bd.* **9**, 101.

MISCH, P., 1949, Metasomatic granitization of batholithic dimensions, *Am. Journ. Sci.* **247**, 209, 372, 673.

MÜGGE, O., 1930, Bewegung von Porphyroblasten in Phylliten und ihre Messung, *Neues Jahrb. f. Min. Beil.-Bd.* **61A**, 469.

NABHOLZ, W. J., 1953, Das mechanische Verhalten der granitischen Kernkorper der tieferen Pennischen Decken bei der Alpinen Orogenese, *C.R. XIX Congr. géol. intern.* (*Algiers* 1952) Sect. iii, 9.

NEWHOUSE, W. H., 1928, The microscopic criteria of replacement in the opaque ore minerals, *The Laboratory Investigation of Ores: A Symposium* (E. E. FAIRBANKS, Editor), 147–63.

NIGGLI, P., 1912, Die Chloritoidschiefer und die sedimentäre Zone am Nordöstrand der Gotthardmassivs, *Beitr. Geol. Karte Schweiz*, N.F. **36**.

1937, *Das Magma und seine Produkte*, Leipzig.

1938, Die komplexe gravitative Kristallisationsdifferentiation, *Schweiz. Min. Pet. Mitt.* **18**, 610.

1942, Das Problem der Granitbildung, *ibid.* **22**, 1.

1946, Die leukogranitischen, trondhjemitischen und leukosyenit-granitischen Magmen und die Anatexis, *ibid.* **26**, 44.

1950, Probleme der Alpine Gesteinsmetamorphose, *Ibid.* **30**, 500.

NOE-NYGAARD, A. AND A. BERTHELSEN, 1952, On the structure of a high-metamorphic gneiss complex in West Greenland with a general discussion on related problems, *Medd. Dansk Geol. For.* **12**, 250.

NOCKOLDS, S. R., 1933, Some theoretical aspects of contamination in acid magmas, *Journ. Geol.* **41**, 561.

1936, The idea of contrasted differentiation: a reply, *Geol. Mag.* **73**, 533.

PERRIN, R., 1935, Le métamorphisme générateur de plissements, *Ann. des Mines*, 149.

AND M. ROUBAULT, 1937, Les réactions à l'état solide et la géologie, *Bull. Serv. Carte géol. Algérie*, 5 *sér.*, *Pétrographie*, no. 1.

1939, Le granite et les réactions à l'état solide, *ibid.* no. 4.

1941, Observation d'un 'front' de métamorphisme régional, *Bull. Soc. géol. France*, 5 sér., **11**, 183.

1945, Observations de métamorphisme du Trias dans les Alpes autochtones au Lac de la Girotte (Savoie), *ibid.* 5 sér., **15**, 171.

PHEMISTER, J., 1948, British Regional Geology, Scotland: The Northern Highlands, *Geol. Surv.*, 2 edition.

PHEMISTER, T. C., 1925, Igneous rocks of Sudbury and their relation to the ore deposits, 34*th An. Rep. Ontario Dept. Mines* Pt. viii, 52.

PITCHER, W. S., 1953*a*, The migmatitic older granodiorite of Thorr District, Co. Donegal, *Quart. Journ. Geol. Soc.* **108**, 413.
1953*b*, The Rosses Granitic Ring-complex, County Donegal, Eire, *Proc. Geol. Assoc.* **64**, 153.
QUIRKE, T. T. and W. H. COLLINS, 1930, The disappearance of the Huronian, *Mem. Geol. Surv. Canada*, no. 160.
RAGUIN, E., 1930, Problèmes tectoniques dans les terrains cristallins du centre de la France, *Bull. Soc. géol. France*, 4 sér., **30**, 51.
1938, Contribution à l'étude des gneiss des Pyrénées, *ibid.*, 5 sér, **8**, 11.
1946, *Géologie du Granite*, Paris.
1949, Aspects de la formation du granite, *Mijnb. Verh. Delft Jaarb.*, 1.
RAMBERG, H., 1948, On the petrogenesis of the gneiss complexes between Sukkertoppen and Christianshaab, West Greenland. *Medd. Dansk Geol. For.* **11**, 312.
1951, Remarks on the average chemical composition of granulite-facies and amphibolite-epidote-amphibolite-facies gneisses in West Greenland, *ibid.* **12**, 27.
READ, H. H., 1923, The geology of Banff, Huntly and Turriff, *Mem. Geol. Surv. Scotland.*
1925, The geology of the country around Golspie, Sutherland, *Mem. Geol. Surv. Scotland.*
1927, The igneous and metamorphic history of Cromar, Deeside, Aberdeenshire, *Trans. Roy. Soc. Edinburgh* **55**, 317.
1931, The geology of central Sutherland, *Mem. Geol. Surv. Scotland.*
1933, On quartz-kyanite-rocks in Unst, Shetland Islands, and their bearing on metamorphic differentiation, *Mineral. Mag.* **23**, 317.
1934*a*, The metamorphic geology of Unst in the Shetland Islands, *Quart. Journ. Geol. Soc.* **90**, 637.
1934*b*, On the segregation of quartz-chlorite-pyrite masses in Shetland igneous rocks, &c., *Proc. Liverpool Geol. Soc.* **16**, 128.
1934*c*, Age problems of the Moine series, *Geol. Mag.* **71**, 302.
1937*a*, Certain aspects of metamorphic geology, *ibid.* **17**, 103.
1937*b*, Metamorphic correlation in the polymetamorphic rocks of the Valla Field Block, Unst, Shetland Islands, *Trans. Roy. Soc. Edinburgh*, **59**, 195.
1940, Metamorphism and igneous action: Presidential Address to Section C, British Association, Dundee Meeting, 1939, *Advancement of Science*, **1**, 223.
1942, Rocks showing felspar porphyroblasts and nodes from Unst, Shetland, *Proc. Geol. Assoc.* **53**, 107.
1943, Meditations on granite: Part One, *Proc. Geol. Assoc.* **54**, 64.
1944, Meditations on granite: Part Two, *ibid.* **55**, 45.
1946, This subject of granite, *Science Progress* **34**, 659.
1948*a*, Granites and granites, *Geol. Soc. Am. Mem.* **28**.
1948*b*, A commentary on place in plutonism, *Quart. Journ. Geol. Soc.* **104**, 155.
1949, A contemplation of time in plutonism, *Quart. Journ. Geol. Soc.* **105**, 101.
1951, Metamorphism and granitization, *Alex. L. du Toit Memorial Lecture, No. 2. Trans. Geol. Soc. S. Africa, Annex.* **54**, 1.
1952, The Geologist as Historian *in Scientific Objectives*, 52. London.
1955*a*, Granite series in mobile belts *in* The Crust of the Earth, *Geol. Soc. Am. Special Paper* **62**, 409.
1955*b*, The Banff nappe, *Proc. Geol. Assoc.* **66**, 1.
REINHARD, M., 1935, Über Gesteinsmetamorphose in den Alpen, *Auszug aus einem Vortrag in der Mijnbouwkundige Vereenining zu Delft*, 39.

REYNOLDS, D. L., 1936*a*, Demonstrations in petrogenesis from Kiloran Bay, Colonsay. I The transfusion of quartzite, *Mineral. Mag.* **24**, 367.
1936*b*, The two monzonite series of the Newry complex, *Geol. Mag.* **73**, 337.
1942, The albite-schists of Antrim and their petrogenetic relationship to Caledonian orogenesis, *Proc. Roy. Irish Acad.* **48**, sect. B, 43.
1943, Granitization of hornfelsed sediments in the Newry granodiorite of Goraghwood Quarry, Co. Armagh, *ibid.* **48**, sect. B, 231.
1944, The south-western end of the Newry Igneous Complex, *Quart. Journ. Geol. Soc.* **99** (for 1943), 205.
1946, The sequence of geochemical changes leading to granitization, *ibid.* **102**, 389.
1947*a*, The granite controversy, *Geol. Mag.* **84**, 209.
1947*b*, The association of basic 'fronts' with granitization, *Science Progress* **35**, 205.
1947*c*, Hercynian Fe-Mg metasomatism in Cornwall: a reinterpretation, *Geol. Mag.* **84**, 61.
ROQUES, M., 1941, Les schistes cristallins de la partie sud-ouest du Massif Central français, *Mém. Serv. Carte géol. France.*
ROSENBUSCH, H., 1877, Die Steiger Schiefer und ihre Contactzone an den Granititen von Barr-Andlau und Hohwald, *Abh. geol. Specialkarte Elsass-Lothringen*, Strassburg.
1896, *Mikroskopische Physiographie.*
1923, *Elemente der Gesteinslehre.*
ROSS, C. S., 1928, Physico-chemical factors controlling magmatic differentiation and vein formation, *Econ. Geol.* **23**, 866.
ROUBAULT, M., 1950, Quelques remarques sur le problème du granite, &c. *Rep. XVIII Intern. Geol. Congr.* (*London* 1948), Part III, 128.
SAHAMA, T. G. ,1935, Zur Regelung der Gesteine im Kristallin von Liverpool-land in Ost-Grönland, *Medd. om Grönland* **95**, no. 6.
1936, Die Regelung von Quarz und Glimmer in den Gesteinen der Finnisch-Lapplandischen Granulitformation *Bull. Com. géol. Finlande*, no. 113.
SANDER, B., 1911, Über Zusammenhänge zwischen Teilbewegung und Gefüge in Gesteinen, *Tscherm. Min. Pet. Mitt.* **30**, 281.
1912, Über tektonische Gesteinsfazies, *Verh. k. k. geol. Reichsanst.*, no. 10, 249.
1923, Zur petrographisch-tektonischen Analyse, *Jahrb. geol. Bundesanst.* **63**, 194.
1930, *Gefügekunde der Gesteine*, Vienna.
SCHEERER, T., 1847, Discussion sur la nature plutonique du granite et des silicates cristallins qui s'y rallient, *Bull. Soc. géol. France*, 2 sér., **4**, 468.
SCHEUMANN, K. H., 1924, Prävariskische Glieder der Sächsisch-Fichtelgebirgischen krystallinen Schiefer, I. Die magmatisch-orogenetische Stellung der Frankenberger Gneisgestein, *Abh. Mat.-Phys. Kl. Sachsisch. Akad. Wiss.* **39**, no. 1.
1932, Über petrogenetische Ableitung des roten Erzgebirgsgneises, *Min. Pet. Mitt.* **42**, 413.
1937, Metatexis und Metablastesis, *ibid.* **48**, 402.
SCHMIDT, W., 1918, Bewegungsspuren in Porphyroblasten krystalliner Schiefer, *Sitzber. Akad. Wiss. Wien Mat.-Nat. Kl. Abt.* 1, **127**, 293.
1932, *Tektonik und Verformungslehre*, Berlin.
SCHOLTZ, D. L., 1946, On the younger Pre-Cambrian granite plutons of the Cape Province, *Proc. Geol. Soc. S. Africa* **49**, xxxv.

SCROPE, P., 1872, *Volcanos*, 2 edition.
SEDERHOLM, J. J., 1891, Studien über archäische Eruptivgesteine aus dem südwestlichen Finnland, *Min. Pet. Mitt.* **12**, 97.
1907, Om granit och gneiss, *Bull. Com. géol. Finlande*, no. 23.
1913, Über die Entstehung der migmatischer Gesteine, *Geol. Rundsch.* **4**, 176.
1923, On migmatites and associated Pre-Cambrian rocks of south-western Finland, I. *Bull. Com. géol. Finlande* no. 58.
1925, The average composition of the Earth's crust in Finland, *ibid.* no. 70.
1926, On migmatites and associated Pre-Cambrian rocks, &c. II, *ibid.*, no. 77.
1928, On orbicular granites, *ibid.*, no. 83.
1932, On the geology of Fennoskandia, with special reference to the Pre-Cambrian, *ibid.*, no. 98.
1934, On migmatites and associated Pre-Cambrian rocks, &c., III, *ibid.*, no. 107.
SENG, H., 1931, Die Gefügeeigenschaften von Granuliten, *Min. Pet. Mitt.* **41**, 453.
1934, Beiträge zur petrographisch-tektonische Analyse des sächsischen Granulitgebirges, *ibid.* **45**, 373.
SHAND, J. S., 1927, *Eruptive Rocks*, London.
SIMONEN, A., 1948, On the petrochemistry of the infracrustal rocks in the Svecofennidic territory of south-western Finland, *Bull. Com. géol. Finlande*, no. 141.
SPENCER, E., 1938, The Potash-Soda-felspars, II. Some applications to petrogenesis, *Mineral. Mag.* **25**, 87.
STAUB, R., 1920, Über Wesen, Alter und Ursachen der Gesteinsmetamorphosen in Graubunden, *Vierteljahrschr. Naturf. Gesellsch. Zürich.* **65**, 323.
STILLWELL, F. L., 1918, The metamorphic rocks of Adelie Land, *Australian Antarctic Exped.* 1911–1914, *Sci. Repts.* ser. A, **3**, Pt. 1.
SUESS, F. E., 1926, *Intrusionstektonik und Wandertektonik im variszischen Grundgebirge*, Berlin.
1929, Gedanken zur Tektonik der schottischen Kaledoniden, *Anz. Akad. Wiss. Wien* **66**, 126.
1931, A suggested interpretation of the Scottish Caledonide structure, *Geol. Mag.* **68**, 71.
1934, Vergleichende Orogenstudien, *Anz. Akad. Wiss. Wien. Jahrg.* **71**, no. 18.
1936, Periplutonische und enorogene Regionalmetamorphose, *ibid.*, *Jahrg.* **73**, no. 23.
1937, Bausteine zu einem System der Tektogenese I: Periplutonische und enorogene Regionalmetamorphose in ihrer tektogenetische Bedeutung, *Fortschr. Geol. Pal.* **13**, Heft 42.
1938*a*, Der Bau der Kaledoniden und Wegener's Hypothese, *Zentralbl. f. Min.* 1938, B. 321.
1938*b*, Bausteine zu einer System der Tektogenese II: Zum Bewegungsbilde des alteren Mitteleuropa: hypokinematische Regionalmetamorphose, *Fortschr. Geol. Pal.* **13**, Heft 43.
SUGI, K., 1931, On the metamorphic facies of the Misaki series in the vicinity of Nakagawa, Prov. Sagami, *Jap. Journ. Geol. Geogr.* **9**, 87.
SULLIVAN, C. J., 1948, Ore and granitization, *Econ. Geol.* **43**, 471.
SUTTON, J. and J. WATSON, 1951, The Pre-Torridonian metamorphic history of the Loch Torridon and Scourie areas in the north-west Highlands, and its bearing on the chronological classification of the Lewisian, *Quart. Journ. Geol. Soc.* **106**, 241.

SUZUKI, P., 1930, Petrological study of the crystalline schist system of Shikoku, Japan, *Journ. Fac. Sci. Hokkaido Imp. Univ.*, ser. iv. **1**, 27.

TEMPERLEY, B. N., 1938, The geology of the country around Mpwapwa, *Tanganyika Terr. Dept. Lands Mines, Geol. Div.*, Short Paper, no. 19.

TERMIER, H. and G. TERMIER, 1953, Géologie et Pétrogénèse, *Bull. Serv. Carte géol. Algérie*, no. 2.

TERMIER, P., 1904, Les schistes cristallins des Alpes occidentales, *C.R. IX Congr. géol. intern.* (*Vienne* 1903), 571.

1912, Sur la genèse des terrains cristallophylliens, *C.R. XI Congr. géol. intern.* (*Stockholm* 1910), 587.

THOMAS, H. H. and W. CAMPBELL SMITH, 1932, Xenoliths of igneous origin in the Trégastel-Ploumanac'h granite, Côtes-du-Nord, France, *Quart. Journ. Geol. Soc.* **88**, 274.

TILLEY, C. E., 1924, The facies classification of metamorphic rocks, *Geol. Mag.* **61**, 167.

1925, A preliminary survey of metamorphic zones in the Southern Highlands of Scotland, *Quart. Journ. Geol. Soc.* **81**, 100.

TUOMINEN, H. V. and T. MIKKOLA, 1950, Metamorphic MgFe-enrichment in the Orijärvi region as related to folding, *Bull. Com. géol. Finlande*, no. 150, 67.

TURNER, F. J., 1930, The metamorphic and ultrabasic rocks of the Lower Cascade Valley, South Westland, *Trans. N.Z. Inst.* **61**, 170.

1933*a*, The metamorphic and intrusive rocks of southern Westland, *ibid.* **63**, 178.

1933*b*, The genesis of oligoclase in certain schists, *Geol. Mag.* **70**, 529.

1934, Schists from the Forbes Range and adjacent country, Western Otago, *Trans. Roy. Soc. N.Z.* **64**, 161.

1935*a*, A contribution to the interpretation of mineral facies in metamorphic rocks, *Am. Journ. Sci.*, 5 ser., **29**, 409.

1935*b*, Metamorphism of the Te Anau Series in the region north-west of Lake Wakatipu, *Trans. Roy. Soc. N.Z.* **65**, 329.

1936, Interpretation of schistosity in the rocks of Otago, New Zealand, *ibid.* **66**, 201.

1937, The metamorphic and plutonic rocks of Lake Manapouri, Fiordland, New Zealand, *ibid.* **67**, 83.

1938*a*, Progressive regional metamorphism in southern New Zealand, *Geol. Mag.* **75**, 160.

1938*b*, Petrofabric investigations of Otago schists, No. 1—Quartz and mica fabrics of three schists from Pateoroa, *Trans. Roy. Soc. N.Z.* **67**, 443. No. 2—Three quartz-albite-sericite schists from Waipori, *ibid.* **68**, 107.

1941, The development of pseudo-stratification by metamorphic differentiation in the schists of Otago, New Zealand, *Am. Journ. Sci.* **239**, 1.

1942, Current views on the origin and tectonic significance of schistosity, *Trans. Roy. Soc., N.Z.* **72**, 119.

1948, Mineralogical and structural evolution of the metamorphic rocks, *Mem. Geol. Soc. Am.* **30**.

AND C. O. HUTTON, 1941, Some porphyroblastic albite-schists from Waikouaiti River (South Branch), Otago, *Trans. Roy. Soc. N.Z.* **71**, 223.

TYRRELL, G. W., 1926, *The Principles of Petrology*.

UMBGROVE, J. H. F., 1947, *The Pulse of the Earth*, 2 edition, The Hague.

VAN HISE, C. R., 1898, Metamorphism of rocks and rock flowage, *Bull. Geol. Soc. Am.* **9**, 269.

1904, A treatise on metamorphism, *Monogr. U.S. Geol. Surv.* **47**.

VIRLET (d'AOUST), 1844, Note sur les roches d'imbibition, *Bull. Soc. géol. France*, 2 sér., **1**, 845.
1847*a*, *ibid.* 2 sér., **4**, 297.
1847*b*, Observations sur le métamorphisme normal et le probabilité de la non-existence de véritables roches primitives à la surface du globe, *ibid.*, 498.
VOGT T., 1927, Sulitelmafeltets Geologi og Petrografi, *Norges Geol. Undersök.*, no. 121.
WAGER, L. R. and W. A. DEER, 1939, The petrology of the Skaergaard intrusion, Kangerdlugssuaq, East Greenland, *Medd. om. Grönland* **105**, no. 4.
WAHL, W., 1936, The granites of the Finnish part of the Svecofennian Archean mountain chain, *Bull. Com. géol. Finlande*, no. 115, 489.
WALDMANN, L., 1927, Umformung und Kristallisation in den moldanubischen Katagesteinen des nordwestlichen Waldviertels, *Mitt. geol. Ges. Wien*, **20**, 35.
1928, Zum geologischen Bau der Thayakuppel und ihrer Metamorphose, *ibid.* **21**, 133.
WALKER, F. and M. MATHIAS, 1946, The petrology of two granite-slate contacts at Cape Town, South Africa, *Quart. Journ. Geol. Soc.* **102**, 499.
WARD, C., 1875, On the granitic, granitoid and associated metamorphic rocks of the Lake-district, Parts I and II, *Quart. Journ. Geol. Soc.* **31**, 568.
1876, On the granitic, granitoid and associated metamorphic rocks of the Lake-district, Parts III–V, *ibid.* **32**, 1.
WATERS, A. C. and K. KRAUSKOPF, 1941, Protoclastic border of the Colville Batholith, *Bull. Geol. Soc. Am.* **52**, 1356.
WATSON, JANET, 1948, Late sillimanite in the migmatites of Kildonan, Sutherland, *Geol. Mag.* **85**, 149.
WEGMANN, C. E., 1935*a*, Zur Deutung der Migmatite, *Geol. Rundsch.* **26**, 307.
1935*b*, Preliminary Report on the Caledonian orogeny in Christian X's Land (north-east Greenland), *Medd. om Grönland* **103**, no. 3.
1935*c*, Über einige Fragen der Tiefentektonik, *Geol. Rundsch.* **26**, 449.
1936, Geologische Merkmale der Unterkruste, *Geol. Rundsch.* **27**, 43.
1938, Geological investigations in southern Greenland Part. I. On the structural divisions of southern Greenland, *Medd. om Grönland* **113**, no. 2.
1947, Note sur quelques problèmes de la tectonique superposée, *C.R. Soc. géol. Finlande*, no. 20, 223.
1948, Remarques sur le métamorphisme régional, *Geol. Rundsch.* **36**, 40.
1951, Méthodes d'analyse tectonique des socles cristallins, *Arch. des Sciences Genève* **4**, 233.
AND E. H. KRANCK, 1931, Beiträge zur Kenntnis der Svecofenniden in Finnland, I, II. *Bull. Com. géol. Finlande*, no. 89.
WEINSCHENK, E., 1901, Mémoire sur le dynamométamorphisme et la piezocristallisation, *C.R. Congr. géol. intern.* (*Paris* 1900) 326.
1902, Vergleichende Studien über den Contactmetamorphismus, *Zeitsch. deutsch. geol. Gesellsch.* **54**, 441.
WELLS, A. K. and S. W. WOOLDRIDGE, 1931, The rock groups of Jersey, with special reference to the intrusive phenomena at Ronez, *Proc. Geol. Assoc.* **43**, 197–8.
WENK, E., 1937, Zur Genese der Bändergneise von Ornö Huvud, *Bull. Geol. Inst. Upsala* **26**, 53.
WILLIAMS, G. J., 1934, A granite-schist contact in Stewart Island, New Zealand, *Quart. Journ. Geol. Soc.* **90**, 322.

WILLIAMSON, W. O., 1935, The composite gneiss and contaminated granodiorite of Glen Shee, Perthshire, *Quart. Journ. Geol. Soc.* **91**, 382.

WILSON, G., 1938, The evolution of the granodioritic rocks of the south-eastern end of the Kapaonik batholith, Yugoslavia, *Geol. Mag.* **75**, 193.

WISEMAN, J. D. H., 1932, A contribution to the petrology of the metamorphic rocks of East Greenland, *Quart. Journ. Geol. Soc.* **88**, 335.

1934, The central and south-west Highland epidiorites: a study in progressive metamorphism, *ibid.* **90**, 354.

INDEX

Aar massif, 385
Abbildungskristallisation, 24; *see* mimetic crystallization
Aberdeenshire, cordierite-sillimanite-gneiss, 39; migmatization in, 33, 37, 38, 39, 322, 389; polymetamorphism in, 306; sillimanite zone, 39; soda-front, 38
Ach'uaine hybrids, 185
Agar, W. M., 37
Agents minéralisateurs, 92, 107, 120; *see also* emanations, ichors, volatiles
Agmatite, 115, 144; agmatite front, 144
Airolo, 285
Aktualismus, *see* uniformitarianism
Albite porphyroblasts, 275–6
Albite-porphyroblast schists, 147, 275–6, 281
Albite-schists, Dalradian, 191, 232, 279–80, 282, 295, 321–2
Albitization, in Alps, 323; syntectonic, 321–3
Alderman, A. R., 37
Alkali metasomatism, in albite-schists, 232, 321–3; in enorogenic metamorphism, 29; in felspathization, 147, 149, 150, 151; in metamorphism, 229–30; in migmatization, 38, 122, 125, 131, 251, 252, 355; relation to sillimanite, 257; source, 397
Allure tangentielle, 245, 316
Almandine garnet zone, 19, 202, 204; on biotite zone, 295.
Alps, 320; felspathization, 37; granitization, 385, 387; metamorphism, 8, 28, 100, 211, 212–3, 282–5, 322–3, 361; reactivation of basement, 385, 387
Altvatergebirge, 320
Alumina-rich subtraction-rocks, 353, 356
Ambrose, J. W., 22, 24
Amphibolite facies, 308, 377, 378, 379
Anatectic batholith, 327
Anatectic granites, 193, 258–9, 326–7
Anatexis, 10, 11, 28, 75, 76, 117–8, 121, 132, 161, 175–6, 182, 183, 327, 328, 370
Anatexites, 258–9
Andalusite, 217–8, 362
Anderson, A. L., 35
Anderson, E. M., 73, 83, 175
Anderson, G. H., 33, 37, 87–8, 147–8, 186
Anglesey, 39
Annakoppie, Namaqualand, 353
Anorthosite, mobilized, 366
Antistress minerals, 216–9, 362
Apennines, 389
Apotectonic magmatic phase, 311–2, 318
Appalachian Granite Series, 392
Aqueo-igneous fusion, 57
Archean *see* Pre-cambrian
Ardara pluton, 366
Ardennes, 222
Arendalite, 250
Arterites, 10, 11, 66, 122–3, 131
Assimilation, 33, 65, 88, 106, 109, 110
Aswan granite, 393
Atomic, radii, 132; sizes, 235, 237; *see* ionic radii, sizes
Aubusson gneiss, 327, 393
Augen gneiss, 36, 122, 131, 147, 149–50, 320–1
Augen granite, 149–50
Augen texture, 320–1
Austrids, 28, 212
Autochthonous granites, 327, 330, 364, 389, 390; *see* Granite Series
Autometamorphism, 120
Auvergne granites, 105

Backlund, H. G., on, anatexis, 11; granite-basalt separation, 398; Granite Series, 333–4. 394–5; ionic radii and granitization, 37, 189, 230, 252; metamorphic history, Taimyr rocks, 284; migmatization, 130–4, 183–4, 252, 397; mobilization, 133, 159, 163, 192, 331, 386, 393, 396; rapakivi granite, 135–6, 138–44, 188, 254–5
Baffin Island, 378
Bailey, E. B. on, albite-schists, 232, 279, 280, 281; carbon impedance, 223; Hutton, 49, 52; metamorphism and folding in Highlands, 31, 295; metamorphic zones, 204, 206; sillimanite, Ross of Mull, 307; stratigraphical order, 24
Balk, R. 40, 41, 226–7, 239, 319–20, 343; *see also* Barth, T. F. W.
Ballachulish slate, 223
Baltimore gneiss, 384
Bamble formation, 32, 244, 252, 313, 393
Banding, and bedding, 34, 244; in gneisses and schists, 10, 13, 14, 17, 23, 34, 240–4; in migmatites, 10, 13–4, 17, 34, 240–2; tectonic, 23, 243–4
Banff Nappe, 389
Barberton, Transvaal, 357, 358, 366
Barbour, G. B., 37, 320
Barlow, A. E., 13
Barr-Andlau granite, 3–4, 388
Barrell, J., 27, 40, 241, 248
Barriers, xvii, 357, 358
Barrois, C., 4, 36, 103, 104, 108, 147, 186–7, 346
Barrow, G., 2, filter-press action, 78–9; granites of S. E. Highlands, 18, 78–9, 309–10; metamorphic zones, 18–20, 40, 202–4, 343; regional metamorphism and granitization, 38, 39, 207, 343
Barrovian metamorphic zones, 18–20, 32, 39, 202–7
Barth, T. F. W., on, abundance of granite and basalt, 70, 173; Bamble formation, 32; classification of rocks, 42, 195, 226–7; diffusion, 233–4; Dutchess County rocks, 23, 33, 37, 41, 206, 218, 226–7, 231–2, 319–20; felspathization, 37; granitization, 33, 130; granitization and regional metamorphism, 40, 41; kyanite on shear-planes, 218; metamorphic zones, 205–6; plutonic series, 343–4; shear-planes in metamorphism, 23, 218, 231–2, 239–40; soda-metasomatism of sparagmites, 38, 229, 252; syntectonic soak, 319–20; tourmaline on shear-planes, 231–2
Basalt, 68, 70, 173, 369, 370–1
Basalt-granite separation, xv, xvii, xviii, 113, 172, 173, 174, 178, 268, 367, 397–8; de Beaumont, 96, 165; Kennedy, 84, 165, 171, 369, 370, 371; Michel-Lévy, 105–6, 165
Basaltic layer, *see* sima
Basements, granitic, 69, 375; granitization and mobilization, xviii, 69, 376–86; migmatites, 128
Basic behind, xvii, 353–4
Basic front, xv, xvii, xviii, 184–6, 230, 253–4, 352, 355
Basic magma, derivation, 70–1, 84; provides Volcanic Class, 165–6, 369–70; relation to granite, 76, 77–82, 172, 176–7; source of emanations, 373, 397
Basic Rocks, abundance, 68–70; association with granite, 184, 185, 253–4, 352–3; in Volcanic Association, 83–4, 166, 369–71; origin, 71, 173; pre-metamorphic, 310, 347, 354, 357, 378, 379
Bas-Limousin, 258–9
Bastin, E. S., 271
Batholiths, characters, 73; Colville, 319; in orogenic belts, 73, 77, 84, 175, 391; intrusive, 327–8; room-problem, 161, 179; Suess on, 106; uprise and schistosity, 246
Beaujolais granites, 105, 109
Beaumont, É. de, 96–8, 162, 165, 171, 193, 325
Becke, F., on, Altvatergebirge gneiss, 320; classification of metamorphic rocks, 6, 32; deformation and crystallization, 287; depth-zones, 6, 7, 198–9, 211; equilibrium, 224, 270; metamorphic convergence, 298; metamorphic facies, 223;

mineralizers, 8; rolled garnets, 285; schistosity on bedding, 6, 245; Volume Law, 6, 198, 214
Bedding, 24, 242–3, 244, 247–8
Bederke, E., 320
Bedford gneiss, 320
Ben Vuroch granite, 18, 309
Berg, G., 8, 304–5, 329
Berthelsen, A., 377, 378
Bertrand, M., 262
Besshi series, Japan, 221
Bethlehem gneiss, 319
Billings, M. P., on, batholiths, 319, 329; bedding in metamorphic rocks, 243; crystallization-sequence, 272; metamorphic history, 218; mimetic crystallization, 239; regional metamorphism and igneous action, 40; ring-structure, 179, 384; tourmaline in metamorphic rocks, 42
Billingsley, P., 253
Biotite enrichment, 184, 191, 253, 352, 353
Biotite zone, 19, 202, 204, 220
Bittesch gneiss, 302, 304, 320
Blair Atholl granites, 309
Boron, origin in metamorphic rocks, 41
Bosworth, T. O., 307
Boué, A., 91
Boule, M., 292
Boundary Peak granite, 147–8
Bowen, N. L., 169, 174; on, crystallization-differentiation, 77–9, 81, 251, 370; diffusion in the solid, 190; filter-press action, 177–8; fronts, 251, 352; granite magma by remelting, 72, 175; mobilization, 192; monomagmatism, 77–9, 81, 171; roomproblem, 179; vein-fillings, 64
Bowes, D. R., 348
Brammall, A., 35, 40, 42, 228–9
Bridgman, P. W., 25
Brittany granites, 336, 337, 391
Broedel, C. H., 384
Broken Hill, N.S.W., 348
Browne, W. R., 319, 329
Bubnoff, S. von, 298, 303, 312, 335
Buchan, Aberdeenshire, 39
Bucher, W., 335–6, 372, 385, 393–4
Bugge, J., banding in gneisses, 244; diffusion in solid, 189–90, 236, 237; migmatite front, 252; synkinematic granites, 313, 393
Bull, A. J., 247
Bunsen, R., 70, 71, 74, 77, 173
Burgersfort, Transvaal, 362
Bushveld complex, 178, 306, 362, 366, 369

Calcareous rocks in granitization, 131, 132; Nanga Parbat, 346; Pyrenees, 109, 110, 346; Stavanger, 204; *see* Limestone
Calc-silicate gneiss, 204–5
Calc-silicate mica-schist, 204–5
Caledonids, Granite Series in, 392, 394; metamorphic history in, 294–6, 321–2; North-west Highlands, 289; Scandinavia, 384, 387; Suess' interpretation, 28, 213–4
Calkins, F. C., 42
Canadian Shield, 33
Cape plutons, xvii, 152–3, 156, 349–50, 365
Carbon impedance, 223
Carn Chuinneag, 282
Cascade Mountains, 368
Cassia batholith, 35
Cataclasis, 22, 196, 288
Cauldron-subsidence, 161, 179, 389, 394, 395; *see* ring-intrusion
Cevennes, 245–6, 257, 313
Chamberlin, T. C., 26
Chapman, C. A., 239
Chelius, C., 4
Chelmsford granite, 320
Chemical gradient, 237, 249, 250
Chemical mobility, 330
Chemistry of granitization, 184, 185
Chemistry of the Granite Series, 333–4, 395–6
Cheng, Y. C., on, crystallization-sequence in migmatites, 270, 273–4, 306, 347–8; felspathization, 150, 186, 347; grain-boundary effect, 236; lit-par-lit replacement, 241
Chlorite zone, 19, 205, 220, 249
Chloritoid, 272, 297, 361–2
Chromite, 366
Circumscribed granites, 193, 326–7, 332, 389
Clarke, F. W., 21, 27

Classification of rocks, Barth, 42, 195, 226–7; Green, 57, 83; Lyell, 53, 83; Read, 162–7, 170–1, 195, 368–71
Clastic mica zone, 19, 202
Cleavage, 246, 247, 248, 278
Cloos, E., on, chloritization, 300; crystallization-sequence, 274, 278; granite domes, 384; metamorphic history, 218, 305; regional metamorphism and intrusion, 212; stoping, 180
Cloos, H., 172, 332
Closed systems, 225
Clough, C. T., 232, 279–80, 281
Cole, G. A. J., 17, 36, 154
Coleman, A. C., 13
Collins, W. H., 33, 69, 233
Colonnes filtrantes, 5, 6, 39, 111–12, 124, 200, 251; *see* emanations, ichors
Colonsay, camptonite, 236–7; hornblendite, 36
Colony, R. J., 271
Colville batholith, 319
Composition changes, in the Granite Series, 333–4; in the Plutonic Series, 41, 224, 226, 232; Lapadu-Hargues on, 191, 227–31.
Compression, 246
Concretion principle, 249
Congeneric granites, 327
Conglomerates, basal, 334, 382, 394
Contact-metamorphic schists, 8–9
Contact metamorphism, 2, 3–4, 12, 101, 106, 107–8, 122, 326, 362, 366
Contact ore-deposits, 12
Contact-relations of granites, French School, 113, 181, de Beaumont, 97–8, Fournet, 100–1, Lacroix, 107, 108, Michel-Lévy, 103–4; German School, 108; of the Granite Series, 325–6, 329–30, 332, 349–50, 364–6
Contacts, 156, 157, 183
Contamination, 33, 35, 65, 67, 88, 174
Continents, discontinuous, xvii, 371–3
Convection currents, 246–7, 396–7; and metamorphism, 247
Copper deposits, Namaqualand, 354
Cordierite, 38, 39, 127, 257
Corin, F., 222
Cornelius, H. P., 212–3
Cornish granites, 336–7, 389, 392
Cornucopia, Oregon, 37
Cotta, B., 12, 100, 101, 151
Cover in plutonism, 27, 29, 211, 212
Cowal albite-gneiss, 279–80
Creative block, 28, 29, 213–4
Credner, H., 323
Critical level, 199
Cromar, Aberdeenshire, 33, 38, 310
Crustal melting, 72–3, 84, 175, 370
Crystal lattice, 190, 236, 237, 238, 277, 329
Crystallization, 269
Crystallization and deformation, 276–88
Crystallization-differentiation, xvii, 69, 77–83, 84, 141, 166, 173, 176–8, 370
Crystallization-sequence in Plutonic Series, 270–6
Crystal settling, 78, 173, 370
Cunningham-Craig, E. H., 232, 281
Current-bedding, 23, 245
Currier, L. W., 254, 256, 320, 329
Cycle, Prime, 268, 367

Dabenoris, Namaqualand, 353
Dalradian Series, 219, 220, albite-gneisses, 232, 321–2; Banff Nappe, 389; chloritoid-schists, 361; depth-zones, 30, 211; inversion of metamorphic zones, 30, 294–6; *see also* Barrovian metamorphic zones.
Daly, R. A., 11, on, earth-structure, 68, 69, 70, 173, 372; granite, and granite magma, 72, 73, 74, 76–7, 80; metamorphism, 15, 21, 22, 23, 25, 26, 27, 41, 175, 176, 177, 195–6, 242, 245; uniformitarianism, 26, 27, 176
Dana, J. D., 12
Darling pluton, 350
Dartmoor granite, 35
Darwin, C., 78, 247–8
Darwin, G., 372
Daubrée, A., 21
Davidson, C. F., 218
Dawson, G. M., 15
Deckgebirge, 199
Deer, W. A., 79, 80, 177
Deformation, *see* paracrystalline, pre-crystalline, post-crystalline

Deformationsverglimmerung, 303
Delesse, A., 94, 101–2, 145, 157, 162, 193, 325, 390
DeLury, J. S., 246
Demay, A., classification of granites, 326, 390; inversion of crystalline schists, 291, 293; magma and tectonics, 212, 245–6, 257, 310, 313–7, 386; magma and regional metamorphism, 212, 257; orogeny and migmatization, 322
Dents de cheval, xv, xvii, xviii, 104–5, 151, 187–9, 348; *see also* felspathization
Depth in metamorphism, 6–7, 14–5, 25–7, 27, 30, 131, 198–202, 203–4, 225–6, 238–9, 289–94, 359; kinds, 210–6
Depth zones, Becke, 6, 198, 299; Grubenmann-Niggli, 7, 197–8, 198–200, 208, 210, 214; Heim, 6; Jung and Roques, 200–2, 290
Deuteric changes, 120
Deville, H. Ste C, 92, 96
Devon granites, 336–7, 389, 392
Diabase derivatives, 216
Diabrochite, 185, 253
Diadysites, 258–9
Diaphthoresis, 220, 299–300, 303, 304, 305, 307, 317
Diapirs, 131, 132, 328, 332, 388, 393
Differential anatexis, 11, 74–5, 175–6, 177–8, 249, 367, 385, 396
Diffusion, xv, 17, 35, 133, 189–90, 233–9, 241–2, 250, 254, 351
Diorite, 173, 174
Dislocation metamorphism, 4, 14–5, 21–2, 251, 298–9, 306, 307
Doming, 161, 178
Donegal, Ireland, 36, 346–7, 357, 365, 366, 393, 395, 396
Dordogne, 259
Downbuckling, xviii, 396, 397
Drachenfels trachyte dome, 318
Drescher, F. K., 333
"Drys", xvi, 237, 351
Dunn, J. A., 185, 253
Du Reitz, T., 37, 40, 218, 274
Durocher, J., 70, 71, 72, 74, 77, 93, 95, 173
Dutchess County, N.Y., *see* Balk, R., and Barth, T. F. W.
Du Toit, A. L., 339–40, 344, 363
Dwars River, Transvaal, 366
Dykes, as deformation indexes, 308, 378, 379
Dynamic metamorphism, 2, 4, 5, 6, 15, 21–5, 42, 105, 358, 359
Dynamometamorphic schists, 9

Early granites, 128, 332
Earth shells, 67, 68, 69, 173
Ebert, H., 330
Ecker gneiss, 306–7
Eckermann, H. von, 138–43, 218, 333, 334
Eclogite, 215–6, 221, 303, 308, 324, 362–3
Ectinites, 200–2, 209, 212, 227, 257–62, 290–3, 360–1
Edelman, N., 377
Edwards, A. B., 271
Eeden, O. R. van, 357, 366
Eitel, K., 236
Elles, G. L., 30, 203, 204, 211, 295
Emanations, 5, 12, 21, 33, 34, 35, 37, 39, 41–2, 64, 88, 91, 101, 110, 112, 121, 124, 133, 134, 148, 159, 160, 181, 182, 183; nature, 35, 75–6, 97, 133–4, 141, 142, 143; source, xvii, xviii, 34, 128–9, 367–8, 373, 397; *see also* ichors
Embrechites, 258–9
Emmons, W. H., 42
Emplacement of granite, 161, 178–80, 332; *see also* eruptive rocks, intrusion
Enclaves, 95, 104–5, 106, 110, 151, 152–4, 187, 188–9, 235
Endomorphism, 100, 104, 107, 109, 115
Enorogenic metamorphism, 29, 296
Entregelung, 286
Epischists, 6
Epizone, 7, 29, 199
Equilibrium assemblages, 208, 224–5, 264, 265–6; attainment, 208, 224–5, 270
Erdmannsdörffer, O. H., 4, 218, 307, 312
Eruptive breccia, 118; *see* agmatite
Eruptive rocks, 48, 59–60, 102, 106–7, 120–1, 156, 157–61, 192, 331–2

Eruptivity, 156
Erzgebirge, 311–2
Eskdale granite, 56
Eskola, P. E., 212, 215, 218, 367; basalt-granite separation, 371; carbon impedance, 223; crystallization-differentiation, 79, 81; differential anatexis, 74–5, 124, 161, 175–6, 397; granite sequence, 312–3, 333, 395; mantled gneiss domes, 381–4; metamorphic differentiation, 11, 124, 244, 249; metamorphic facies, 207–10, 220, 222, 224, 225; metasomatism, 38; pore-magma, 132, 235
Etna, 54
Exomorphism, 100, 104
Exner, C., 385
Expansion Theory of Orogeny, 25
Exudation, 10, 11, 66, 123; *see* lateral secretion

Fairbairn, H. W., 215–7
Fallot, P., 96
Felspars, large, in country-rocks, 36–7, 100, 104–5, 138, 144–7, 148, 148–50, 151–6, 187–9, 348–9; in granite, 135–8, 149, 150, 187, 318–9
Felspathization, 4–5, 36–7, 89, 91, 100–1, 104–8, 111, 133–44, 145–56, 186–9, 348, 364
Fenner, C. N., 13, 33, 35, 36, 79, 80, 177, 188, 241
Fennoskandian School, 113–34
Fermor, L. L., 215
Fernando, L. J. D., 145, 149–50, 186, 188
Fettke, C. R., 37
Field-work, xi, xii, 92, 108, 110, 170, 178, 186, 340, 345
Fig Tree Series, Swaziland, 357
Filter-press action, 18, 19, 78, 82, 177–8 ,309, 331
Finland, Pre-cambrian, 9, 10, 29, 31, 37, 38, 69, 113, 313, 376–7, 381–4
Finnish Granites, 333, 334, 395; *see* rapakivi
Flamanville granite, 103, 106, 151, 187
Flett, J. S., 21, 282
Flin Flon, Canada, 22
Flow-structure, in eruptive rocks, 318–9, 320; in migmatites, 377, 379–80, 385
Foliation, in metamorphites, 5, 12, 13, 16, 17, 35, 247, 248, 364; in granites, 319–20
Fourmarier, P., 396, 397
Fournet, J., 91, 93, 98–101, 145, 146, 151, 325
Franciscan series, 363
Frankel, J. J., 349
Frankenberg gneiss, 311
French School, 4–6, 33, 36, 90–113, 145, 147, 162, 165, 181, 193, 200, 345–6; work summarized, 112–3, 181
Friedeberg gneiss, 333
Friedel, E., 240
Fronts, xv, xvi, 5, 37, 38, 40, 111, 124, 126–7, 134, 151, 182, 183, 184, 190–1, 250–5, 352; *see* basic front, migmatite front

Gäbert, C., 4
Galloway granites, 36, 306, 330, 389, 392
Garnet, 207, 214–5, 356; rolled, 282–5
Garnet zone, 19, 202, 204, 207
Gas fluxing, 14, 35, 36
Gefügekunde, 277
Geijer, P., 312, 329
Geikie, A., 21, 58
Geikie, J., 58
Geochemical culmination, 185
Geosynclinal phase of magmatic activity, 172
Geosynclinal sediments, 26, 73, 128, 130–1, 211–2, 375–6, 388–9
Gevers, T. W., 339, 340, 344, 349
Gföhl gneiss, 308, 321, 324
Ghost stratigraphy, 104, 106, 110, 132, 180, 183–4, 345–6
Ghost structure, 132, 180, 183–4, 345–7, 393
Gilluly, J., 41
Gindy, A. R., 346–7, 357, 393, 396
Glaucophane-schist, 221, 363
Glen Muick, 310
Glen Shee, 42

Glen Tilt, 52, 309
Gneiss, 5, 226, 259–61, 313–4; Alpine, 8; banded, 34, 35; formed by felspathization, 108, 122, 149; glandaleux, 105; lit-par-lit, 240–1; migmatitic, 10, 11, 12, 13, 16, 17, 18, 19; mobilization, 364; permeation, 66; veined, *see* venites
Goguel, J., 247, 250, 262
Goldschmidt, V. M., 33, 40, 41, 121–2, 146, 147, 186, 204, 206–7, 211, 227, 232, 256, 359–60
Goodspeed, G. E., 37
Goranson, R. W., 41
Gowganda, Ontario, 192
Graded bedding, 23
Grampian Highlands, 30, 202, 203; *see* Highlands, Scottish
Grampian granites, 18, 392
Granite, *passim*, especially, xii, 45–8, 49–58, 70, 87–112, 135–62, 174–93, 308–37, 363–9, 374–96; defined, 45–8, 174, 341; early discussion on, 49–58; melting of, 73; metamorphic theory of, 55–6, 56–8, 102, 164–5, 170
Granite Series, especially, xiv-xviii, 98, 193, 206, 259, 262, 308–13, 324–37, 351, 363–7, 374–5, 384, 390–96; change of composition in, 333–4, 396; defined, 325; time-span of, 394
Granitic layer, *see* sial
Granitic magma, xiii, xiv, 10, 54–5, 66, 67, 84, 106, 109, 112, 117–8, 161, 178–80, 181, 182, 309–13, 332, 370; origin, 13, 33–34, 70–83, 86, 173, 175–8, 370; primary, 33, 175–6
Granitification, 94, 102
Granitization, *passim*, especially, xiii-xviii, 5, 16, 32–7, 76, 84, 87–9, 91, 97–8, 103–4, 106–7, 114–6, 124, 125–7, 131–4, 139, 141, 145, 151, 156, 157–9, 163, 174, 180–6, 330–1, 342–51, 363–4, 367–8, 388–9; and eugeosynclines, 397; and mobility, 378, 379, 380, 381, 384, 389; criteria, 148; definitions, 87–9, 114–6, 145, 174, 180–1; examples, 103–4, 124, 125–6, 139, 141, 146–51, 342–4, 346–9; general remarks, xiii-xviii, 32–7, 97–8, 156, 157–9, 163, 181–6, 330–1, 344–51, 363–4, 367–8, 388–9; process, 5, 76, 91, 106–7, 126, 131–4, 174, 181–4, 351; scale, 349, 351, 389–90; *see also*, metamorphism, migmatization, mobilization.
Granit-tektonik, 180, 332, 337, 365, 392
Grantham, D. R., 152, 154, 156, 188
Granulite facies, deformation in, 377, 378
Granulites, 303, Saxon, 323–4
Graphic granite, 97, 270–1
Graton, L. C., 64, 79, 88, 237
Green, A. H., 16, 57–8, 83, 158, 161, 325
Green, J. F. N., 41
Greenland, 35, 37, 79, 127, 129, 144, 149, 186, 189, 288, 313, 330, 377, 378
Greenly, E., 2, 16, 17, 206, 241, 256; *see* Horne, J.
Greenschist facies, 221–2, 224–5
Greenstone, resisters, 183
Greig, J. W., 73
Greisen, 97
Greywacke, metamorphic zones, 205
Griggs, D., 247, 396
Groddeck, A. von, 12
Groot Paternoster, Cape Province, 350
Grout, F. F., 37, 61, 80, 90, 95, 177, 186
Grubenmann-Niggli depth zones, 7, 30, 199, 201
Grubenmann, U., 7, 130, 197–8, 199–200, 225–6, 239
Grundgebirge, 199
Guéret granite, 327, 393
Gullkrona basin, Finland, 377
Gürich, G., 4.

Hales, J. V., 247
Haliburton-Bancroft area, Ontario, 13
Hall, A. L., 366
Hangö granite, Finland, 38, 114, 116, 124–5, 127, 132, 157–8, 182, 251, 252, 313, 377

Harker, A., 39, 46, 72, 157, 226, 233, 294, 307; felspathization, 36, 146, 154, 187; foliation, 243, 248; metamorphic zones, 203, 206, 211; rapakivi ovoids, 137; regional metamorphism, 21, 256; Shap granite, 152, 154, 155; stress minerals, 217–8
Harris, Hebrides, 218
Harvey, R. D., 271
Hatch, F. H., 46–7
Haughton, S., 70
Heim, Albert, 6
Heim, Arnold, 26
Helicitic structure, 279, 283, 284, 286
Hercynian belt, Central Europe, 213–4, 298, 301–5
Hercynian Granite Series, 335–7, 391–2, 394
Hess, H. H., 222
Heterogenization, 249–50
Hietanen, A., 207, 212, 218, 274–6, 278, 281, 300, 305
Highlands, Scottish, 264, 266, 282, 301; North-west, 288, 289, 296, 300, 306, 308, 379–80, 387, 392; South-east, 18–20, 30, 202, 294, 295, 309–10, 343, 361; South-west, 204, 232, 279, 281, 282, 321–2; *see also* Aberdeenshire, Sutherland
Hinterlechner, K., 303
Hogbom, A. G., 10
Hohe Tauern, Austria, 299, 385
Holmes, A., 46, 60, 72, acid magma generated by basic intrusion, 73–4, 175; convection currents, 247, 396; crystallization-differentiation, 80, 82, 177; diffusion, 234; emanations, 253, 397; granitization process, 37
Holmquist, P. J., 10, 11, 21, 66, 67, 74, 121, 122–4, 130, 137, 175, 249
Holser, W., 238, 253, 255
Holtedahl, O., 385
Homogenization, 161, 192, 249, 250
Horizontal schistosity, 23, 245
Horne, J., 16, 206, 256
Hornfels, 330, 362
Hudson River Slate, 343–4
Huron, Lake 36
Hutchison, A., 203
Hutton, C. O., 205, 222, 232, 233, 281, 288
Hutton, J., xii, xix, 6, 44, 49–52, 54, 56, 85, 90, 117, 119, 161, 169, 197
Hyperkinematic granites, 313
Hypokinematic metamorphism, 28

Iceland, 70
Ichors, 10, 35, 75–6, 119–21, 124–5, 131, 132, 133, 141, 158, 182, 189, 215, 309
Idaho batholith, 254
Iddings, J. P., 47
Igneous, Hutton on, 51; Lyell on, 52–4, 56; defined, 60–1
Igneous action, 54
Igneous facies, 208
Igneous fusion (Ward), 57
Igneous rocks, xii, xiv, 6, 48, 52–3, 54, 59–61, 63, 64, 65, 83, 86, 166, 174, 341, 369
Imbibition, 4, 34, 93, 96, 99, 101, 103, 107, 108, 110, 116–7, 126, 149; *see*, permeation
Inchbae granite, Ross-shire, 306
Inclusions, *see* enclaves
Index minerals, 19
Infrastructure, 251–2, 386–7
Ingerson, E., 279
Injection, 4, 14, 18, 33, 88, 89, 101, 103–4, 107, 108, 110, 122–3, 240, 241; defined, 126
Injection complex, 13–4, 16–7, 34, 122–3
Injection gneiss, 318, 322
Injection metamorphism, 122, 207, 218, 227, 256
Intergranular film, 126, 134, 159; *see* pore-liquid
Intrusion, 106–7, 132, 157–61, 178–80, 192, 331–2, 387; defined, by Wegmann, 124, 126
Intrusion tectonics, 213, 301
Intrusive rocks, 56–7, 58, 84, 124, 127–8, 178–80, 327–8, 337, 365, 383–4
Inversion of metamorphic zones, 27, 30–1, 39, 40, 204, 205, 211, 288–95
Inyo Mountains, California-Nevada, 33, 37, 87–8, 147–8, 186
Ionic migration, 189–90, 191, 230–1, 238, 351
Ionic packing, 215

Ionic radius, 37, 191, 230–1, 252
Isochemical series, validity, 32, 40; packing index, 216
Isograd, 31, 203, 209
Isophysical Series, 208, 209, 221
Isostasy, 68
Isotherms, rise of, 39

Jacobson, R. R. E., 179
Jamestown Series, Swaziland, 357
Jamieson's Drift, Natal, 344
Jamotte, A., 240
Jeffreys, H., 247
Johannsen, A., 47, 61, 136
Johnson, C. H., 254
Joly, J., 25
Jones, O. T., 26
Jotnian diabases, 139, 141
Jotnian sandstone, 135, 139, 140, 254
Judd, J. W., 14, 22, 57, 242
Jung, J., 191, 200–2, 212, 219, 227, 235, 246, 257–62, 290–1, 327, 360–1, 390, 393

Kaap granite, Transvaal, 366
Kantorp, Sweden, 37
Kapaonik granodiorite, 40
Karelids, 381, 384, 387
Karpoff, R., 218
Katamylonite, 321
Katazone, 7, 28, 199, 303
Kautsky, G., 384–5, 393
Kellerjoch gneiss, 299
Kemp, J. F., 21
Kennedy, W. Q., 83–4, 97, 165–7, 171, 295, 369
Kieslinger, A., 299
Killiney contact-schists, 330
King, H., 348
Kinzigite, 251, 252, 254, 272
Klemm, G., 4
Klüpfel, W., 172
Knopf, A., 312
Knopf, E. B., 278, 299, 303
Koenigsberger, J., 8, 9, 38, 304, 328
Köhler, A., 323
Kölbl, L., 299, 303, 320, 323
Kossmatt, F., 310
Kranck, E. H., 37, 124, 125, 182–3, 239, 251, 310, 313, 377, 378
Krauskopf, K., 319
Krige, L. J., 282–4
Krokström, T., 80, 177
K-values, 334, 396
Kyanite, 218–9, 272, 353
Kyanite zone, 19, 202

Lacroix, A., 4, 36, 103, 107–10, 112, 114, 193, 326, 346
Lake District granites, 16, 56–8, 158, 392
Lake of the Woods, Ontario, 12
Lamprophyres, 65, 67
Land's End granite, 318
Lane, A. C., 11, 74, 175
Lapadu-Hargues, P., 191, 229–31, 254, 260, 267, 291–3, 355, 356
Lapland granulites, 278, 313
Lapparent, J. de, 3, 47, 60, 156, 190, 220, 295
Larsen, E. S., 25, 26
Late kinematic granites, 313, 395
Lateral secretion, 10, 11, 121, 123, 249; *see* metamorphic differentiation, exudation
Lattice orientation, 277
Lausitz granite, 330
Lava-granite transition, 51, 54, 172
Lawson, A. C., 12, 13
Laxford zone, 387
Lehmann, J., 4, 5
Leinster granite, 330, 392
Lepsius, R., 4, 7, 329
Level in granites, 351
Lewisian complex, 289, 300, 306, 308, 379–80
Limbrock, H., 303, 323
Limestone, enclaves in granite, 109–10; in granitization complexes, 131–2, 159, 183, 346; reaction with pelites, 233–4; resisters, 331, 346, 347, 357
Lindgren, W., 12, 13, 271
Lit-par-lit structure, 4, 14, 34, 103, 107, 240–2
Load metamorphism, 14–5, 22, 23, 25–7, 242, 245, 359
Loaded zone, 28, 29, 213–4
Local metamorphism, 20

Locke, A., 253
Loewinson-Lessing, F., 71, 173, 329
Longwell, C. R., 247
Lonsdale, K., 236
Loos-Hamra district, Sweden, 139–42
Lossen, K. A., 4, 21, 22
Lot, Massif Central, 291
Lyell, C., xiii, 1, 2, 7, 49, 52–6, 72, 83, 161, 163–6, 170–1, 172, 198

MacCulloch, J., 54
MacGregor, A. M., 329
MacGregor, Malcolm, 33, 36, 88, 89, 90, 142, 159–60, 192, 393
Magma, xii, xiv, and limestone syntexis, 109–10; defined, 60–3; granitic, origin of, 33–4, 54–5, 63–5, 67, 70–82, 86, 161, 176–8; in Granite Series, 332–3; in metamorphism, 211–2; in migmatization, 4, 10, 66, 128, 130, 174; parent, 84, 176
Magmatic cycles, 309
Magmatic segregations, 95
Magnesia metasomatism, 38, 125, 182–3, 251, 252
Magnesium front, 127, 251
Magnusson, N. H., 37, 132, 252
Malingsbo granite, 38, 252
Malmesbury slate, 152, 153, 349, 350, 361
Mantled gneiss domes, 381–4
Margeride, Massif Central, 291, 322, 327
Marr, J. E., 152, 155
'Martic Overthrust', 207, 274–5, 278, 281, 305
Maryland domes, 384
Massif Central, 200–2, 212, 245–6, 257–62, 290–3, 313–8, 322, 327–8, 336, 337, 360–1, 391, 393, 395
Mathias, M., 188
Mayo, E. B., 180, 253
McCallien, W. J., 11, 24, 41, 232
Mfongosi series, Swaziland, 344
Mechanical mobilization, 331
Meinesz, F. A. Vening, 247, 396
Melting of crust, 72, 84, 175
Melting-up, 126
Merwin, H. E., 73
Mesozone, 7, 29, 199
Metadolerite, 331, 347, 357
Metamorphic convergence, 264, 266, 297, 298, 300, 302–4, 305
Metamorphic correlation, 203, 223–4
Metamorphic differentiation, 11, 32, 74, 124, 243, 244, 247–50, 353, 356
Metamorphic diffusion, 11, 32, 74, 124, 189–90, 221, 233–42, 254–5, 356
Metamorphic facies, 207–10, 220–5, 265–6, 356
Metamorphic front, 190
Metamorphic history, 208, 218, 219, 296–305
Metamorphic orogeny, Theory of, 25
Metamorphic zones, xiv, xvii, 30–2, 39–40, 100, 190–1, 197–207, 220, 256–62, 290–5, 343–4, 356, 358–9; composition effects on, 32, 204, 221–3, 227–33, 355–6; *see* Barrovian metamorphic zones, depth zones, ectinites
Metamorphism, *passim*, defined, 2, 195; *see* contact metamorphism, dislocation metamorphism, dynamic metamorphism, load metamorphism, local metamorphism, orogenic metamorphism, regional metamorphism, granitization, migmatization
Metasomatism, 12, 33, 34, 37–8, 40, 42, 88, 119, 121, 122, 124–5, 126, 128, 131–2, 133, 142, 147, 150–1, 152–3, 184, 190, 225–7, 237–9, 250–5, 342, 351–2, 355, 357, 359, 378
Michel-Lévy, A., 4, 5, 38, 94, 103–7, 108, 109, 110, 112, 114, 151, 162, 165, 171, 187, 193, 200, 240–1, 326, 346
Migma, 33, 63, 64, 128, 130, 131, 160, 192, 309, 331–2
Migmatite, *passim*, 10, 39, 42, 63, 65, 66, 67, 115, 122, 130, 182, 196, 200, 202; *see* granitization, migma, migmatite front, migmatite zones, migmatization
Migmatite front, 111, 112, 125–8, 131–2, 139, 183, 191, 202, 212, 251–3, 254–5, 257–62, 379–80
Migmatite zones, 257–62

Migmatization, *passim*, xvi, xvii, 28, 33, 34, 35, 39, 40, 84, 88, 115–21, 124–9, 130–2; superposition on metamorphism, 206–7; *see* granitization, migma, migmatite, migmatite front, migmatite zones
Migration, 126
Mikkola, E., 114
Mikkola, T., 377
Milch, L., 14, 23, 32, 34, 36, 242
Miller, H., 16, 33, 34
Millevaches granite, 328
Mimetic crystallization, 24, 28, 40, 240, 242, 277, 286, 287, 295
Mineralizers, 11, 92, 104, 107, 108, 120, 123, 181, 235
Misaka series, Japan, 221
Misch, P., 343, 346, 360, 361, 388, 397
Mobile belts, xviii, 374, 375
Mobilization, xvii, 57, 58, 63, 102–3, 124, 131, 133, 134, 156, 157–61, 182, 192, 325, 331–2, 354, 358, 365, 366, 377, 384–5, 392–3, 393–4
Moine series, 28, 29, 214, 271–2, 288, 296, 298
Moine Thrust, 296, 298
Moldanubicum, 28, 29, 301–5, 307, 323–4
Monazite, 353
Monomineralic rocks, 271–2
Montagne Noire, 258–9, 290
Mont Blanc granite, 105
Moodies series, Swaziland, 357, 358
Moon, origin of, 372
Morar, Scotland, 39
Moravicum, 28, 29, 301–5, 307
Morvan, Massif Central, 105, 259–60
Movement of material in plutonism, 237
Movement-picture, 277, 282, 307
Münchberg gneiss, 305
Muness phyllites, Unst, 24
Muruhatten, Sweden, 274
Muscovititis, 29, 303, 304
Mylonites, 5, 9, 22, 288, 291, 300, 318, 319, 379

Nabholz, W. J., 385
Namaqualand, xvii, 349, 353–4, 356, 364, 365
Nanga Parbat, 343, 346, 360
Natal, xvii, 349, 356, 364
Naumann, C. F., 1
Nel, L. T., 349
Nelspruit granite, 357, 358, 364, 366
Neptunic Class, 166, 171, 371
Neptunists, xii, 49
New Hampshire, 42, 179, 272, 384
Newhouse, W. H., 271
New Jersey gneisses, 13
Newry granodiorite, 150–1, 184, 186, 253, 254, 352–3
New Zealand, 37, 42, 220, 232, 249, 272–3, 281
Nigeria, 179
Niggli, P., on, crystallization-differentiation, 78, 79, 80; diffusion, 233; granitization, 81–2, 98, 155, 171, 187, 333, 395; magma, 61–2, 63, 65, 80, 81–2, 177; metamorphism, 7, 146, 199–200, 208–9, 212, 233, 280, 287, 361; migmatization, 66–7, 89, 116, 129–30, 146, 250, 252
Nockolds, S. R., 35, 78, 81, 233
Noe-Nygaard, A., 377, 378
Nyanzian System, 387

Oberbau, 127, 251–2, 387
Octoraro schist, 275
Odenwald, 298
Old Granite (South Africa), 364
Old magma (Barrow), 18–20, 39, 203, 309–10, 328, 343
Oligoclase-gneiss, 309, 310
Oligoclase zone, 204, 205, 223
Oliverian magma series, 384
One-magma Theory, 77–82, 176–8, 370–1
O'Okiep copper field, 354, 365
Open systems, 224, 225–7, 227–33
Orange River, 353
Ore-deposition, 12, 329, 354
Original composition effect, 220–4
Original structures, in metamorphic rocks, 23–4, 31–2, 245, 355, 358–9; in migmatites, 10, 16–7, 34–5, 58, 105, 125–6, 132, 346, 348
Orijärvi, Finland, 38, 127, 251, 312, 333, 377
Ornö, Sweden, 243

Orogenic metamorphism, 360, 361, 362–3, 367, 388
Orogeny, and magma, 73, 74, 76, 77, 84, 176, 309–21, 370; and plutonism, 22, 28–9, 84, 119, 125, 172, 246, 261–2, 289–96, 309–24, 360–1, 367, 371, 375–85, 391–6, 396–8
Ortsregelung, 243–4
Osborne, F. F., 235
Otago, New Zealand, 281
Overprinting, 278
Overtaking, 332

Paardeberg pluton, 350
Packing Index, 215–6, 217
Palingenesis, 10, 11, 33, 34, 75–6, 76, 88, 89, 117, 118, 119, 123, 131, 158; *see* anatexis
Paracrystalline deformation, 278–85, 286, 287–8, 294
Paramorphism, 363
Paratectonic crystallization, 29
Parautochthonous granites, 365, 390
Peach, B. N., 17
Pegmatites, 10, 11, 74, 97, 122–3, 124, 132–3, 144, 182, 218, 300, 309–10
Pekeris, C. L., 247
Pelites, 18–9, 32, 116, 122, 131, 132, 152, 183, 185, 202–3, 226, 229–31, 297–8, 347
Pelletier, R. A., 361
Pellinge, Finland, 127, 136
Pellisier granite, 147–8
Pennids, 28, 212
Peridotite magma, 72
Periplutonic metamorphism, 28, 296
Permeation, 4, 10, 14, 16–7, 18, 33, 34, 35, 88, 93, 101, 111, 116–7, 119–21, 124, 125, 126, 136, 143, 182, 239, 240, 241, 321–3, 344, 351, 355, 364
Permeation-gneiss, 66, 150, 347
Perrin, R., 25, 189, 190, 235–6, 237, 241, 242, 250, 255, 274
Petrofabrics, 277–9
Pfahlschiefer, 298
Phemister, J., 296
Phemister, T. C., 64
Philipp, H., 4
Phillips, F. Coles, 203
Phyllonitization, 220, 299–300, 304
Piezocontact metamorphism, 8
Piezocrystallization, 8, 9
Pitcher, W. S., 346–7, 395
Pitkaranta, Finland, 382
Plagioclase-porphyroblast schists, 207
Plastic deformation in migmatites, 377, 378–9, 380, 381, 385
Plastic zone, 384, 385
Plutonic action, 101–2, 119, 163–5, 170, 267
Plutonic Association (Kennedy), 83–4, 97, 105, 113, 171, 369–70
Plutonic Class or Series (Read), xv, 165–7, 171–4, 190, 195–7, 253, 255–6, 269, 330, 342–4, 356, 368, 370–1, 375, 381, 388
Plutonic Rocks (Lyell) 53, 54, 55, 57, 163, 164
Plutonism, xvi, 195–7, 255
Plutonists, xii, 49
Plutonites, 130, 132
Plutons, 332, 349, 350, 365, 366, 367, 368, 388, 389, 391
Pofadder, Namaqualand, 353
Polymetamorphism, 29, 220, 267, 295, 297, 306–8
Popoff, B., 137
Pore-liquid, pore-fluid, pore-magma, 33, 35, 36, 37, 126, 127, 129, 132, 134, 159, 160, 192, 235, 236, 237, 249, 251, 254
Portsoy, Hutton's pegmatite, 50
Post-crystalline deformation, 280–1, 286, 288, 294
Post-kinematic granites, 313, 395
Post-orogenic sediments, 388, 389–90
Post-tectonic crystallization, 25, 28
Potash-front, 38, 127, 251
Potassic granites, 333, 395–6
Pre-cambrian conditions, 26, 27, 76, 176
Preclik, K., 303
Pre-crystalline deformation, 280–1, 286–7, 294
Preferred orientation, 277
Pre-orogenic sediments, 388–9
Pressure in metamorphism, 214–6
Primary Rocks, 53, 83, 163, 165
Privileged paths, 35, 142–3, 150, 239–42, 243, 248, 313–4
Progression in metamorphism, 31, 202–3, 268

Protoclastic structures, 318–9, 320, 332
Prototectonic magmatic phase, 311–2
Puy-du-Dôme, 109
Pyrenees, 4, 107, 109, 110, 260, 261
Pyrometasomatism, 12, 13

Quartzite, 131, 132, 149, 159, 183; resisters, 104, 331, 347, 357; xenoliths, 36, 236
Queensland, 389
Quérigut granite, 393
Quirke, T. T., 33

Raguin, E., 172, 187, 188, 193, 240, 246, 260, 261, 310, 318, 326–7, 381, 386, 390, 393
Rainy Lake, Ontario, 12
Ramberg, H., 378, 397
Ramsau Line, 298
Rand, 361–2
Rapakivi granite, 35, 116, 135–45, 151, 187–8, 254–5, 313, 334
Rätan granite, Sweden, 139, 140
Reactivation in basements, 378–85
Reactions of granite and wall-rock, 4, 14, 35, 97–8, 99–100, 101–2, 103–4, 106–8, 136
Reade, T. Mellard, 25
Refusion, 117, 119, 121, 182; *see* anatexis, differential anatexis
Regional metamorphism, *passim*, general, 21–32, 127–8, 162, 197–324, 342–4, 354–68, 386–9; and depth, 6–7, 14–5, 25–7, 197–202, 210–6; and migma-magma, 4–6, 15–6, 18–20, 37–42, 190–1, 202–3, 225–33, 250–62, 308–24, 324–7, 342–4, 354–9; and stress, 4, 5, 22–5, 216–9, 276–96, 361–2
Reinhard, M., 63, 212, 385
Replacement, 34–5, 37, 347; textures, 270, 270–1
Resisters, xvii, 104, 183, 240, 331, 347, 353–4, 357–8, 381
Reticular density, 240, 243
Retrogression, 29, 299
Reynolds, D. L., 36, 150, 151, 160, 178, 181, 184, 185, 186, 187, 189, 190, 191, 192, 232, 236–7, 253, 254, 295, 321–2, 352, 393
Rheomorphism, 34, 133, 159; *see* mobilization
Rhodesia, 387
Riecke, E., 22
Riecke's principle, 237
Riesengebirge, 304
Ring-intrusions, 179; *see* cauldron subsidences
Rittmann, A., 172, 372
Roches d'imbibition, 4
Ronez granite, Jersey, 137, 153
Room problem, 34, 133, 141, 161–2, 178–80, 184, 332
Roques, M., 191, 200–2, 209, 212, 219, 227, 241–2, 246, 257–62, 267, 290–1, 293, 318, 327–8, 334, 360–1, 390, 393
Rosenbusch, H., 3, 21, 22, 47, 61, 103, 104–5, 107–8, 225
Ross, C. S., 64
Ross of Mull granite, 307
Rostrenon granite, 36, 147, 186; *see* Barrois, C.
Rotation of porphyroblasts, 24, 278–86, 287
Roubault, M., 189, 190, 235–6, 237, 241, 250, 255, 274, 367
Rouergue, Massif Central, 258, 291, 322

Sahama, T., 278, 286
Sammelkristallisation, 320, 321
Sander, B., 24, 25, 239, 277, 285, 300
Sauer, A., 4
Saxa Vord, Unst, 297,307
Saxony, 311–2
Scandinavian Hochgebirge, 29, 121–2, 204, 211, 384
Scheerer, T., 62, 96
Scheumann, K. H., 66, 116, 130, 311–2
Schistosity, 248, 276–7; coincident with bedding, 6, 13, 15, 23, 40, 242–5, 290, 355, 358; flat-lying, 6, 15, 245–7, 290; in meta-igneous rocks, 244–5
Schmidt, W., 243, 284–5, 287
Scholtz, D. L., 349, 350
Schuchert, C., 26
Schwenkel, H., 4

Scottish Highlands, *see* Highlands, Scottish
Scourie, 379–80, Dyke, 308
Scrope, P., 62–3
Sea Point contact, Cape Town, xvii, 152–3, 188, 349–50
Sederholm, J. J., 9, 10, 22, 23, 33, 38, 65–6, 69, 75–6, 103, 113–21, 130, 131, 132, 135, 136, 137, 138, 158, 161, 163, 181–2, 188, 196, 198, 214–5, 308, 376
Sedimentary structures preserved, *see* original structures
Selective refusion, 11, 34, 74–5, 124, 161, 175–6, 249, 385; *see* anatexis, differential anatexis
Seng, H., 320, 324
Sequences of crystallization, 269–76
Seve Nappe, 384
Shackleton, R, M., 379–80
Shand, S. J., 59, 60
Shap granite, 37, 56, 152, 155, 156. 188
Shells of Earth, 67–70
Shepherd, E. S., 73
Shetlands, 54, 186, 188; *see* Unst
Shikoku, Japan, 220
Shimmer-aggregate, 29
Shuswap terrain, 15
Sial, 68, 69, 70, 72, 75, 76, 84, 173, 175, 370, discontinuous; 371–3
Sidobre granite, 328, 334
Sierra Nevada pluton, 180
Silesia, 298
Sillimanite, 16, 18, 39, 218-9, 257
Sillimanite-corundum rock, 353
Sillimanite-gneiss in aureoles, 306–7, 330
Sillimanite zone, 18, 19, 27, 39, 202, 203, 206, 343
Sima, xvii, 68, 69, 70, 72, 75, 76, 81, 173, 367, 372, 376, 396–7
Simonen, A., 395
Simultaneous crystallization, 270–1
Sinha, R. C., 396
Skaergaard intrusion, 79
Skarn, 12
Skaw granite, Unst, 37, 154, 156, 188
Skiddaw, granite, 56, slates, 57
Slates, 361
Slip-planes (bedding), 245
Smith, W. Campbell, 36, 137, 153–4
Snowball garnets, 282–5, 287
Soda front, 38, 127, 251, 252
Soda transfer, 38
Södermanland gneiss, 38
Sodic granites, 333, 395–6
Solid diffusion, 189–90, 235–8, 241–2, 254–5, 351
Southern Uplands granites, 58, 392
Sparagmites, 38, 229, 252
Spencer, E., 152–3
Static metamorphism, 14, 15, 22, 23; *see* load metamorphism
Staub, R., 211, 212
Staurolite-schist, 218
Staurolite, trends in, 282, 283, 284
Staurolite zone, 19, 32, 202
Stavanger, Norway, 33, 36, 38, 41, 121–2, 127, 147, 186, 206–7, 227, 251, 256, 359–60
Steinmann, G., 4
St. Gotthard, 102, 280–1, 282
Stillwell, F. L., 11
Stoping, 179–80
Stress in metamorphism, 24–5, 42
Stress minerals, 216–9, 362
Subsequent batholiths, 319, 329
Subtraction rocks, xvii, 353, 354
Sudbury intrusion, 178
Suess, E., 106, denudation series, 172
Suess, F. E., 28–9, 30, 213–4, 218, 295–6, 301–4, 320, 322–3, 329–30, 335, 386
Sugi, K., 218, 221, 222
Sui generis, the motto of the metamorphic geologist, 358
Sulitelma Mountains, Norway, 205, 223, 293–4
Sullivan, C. J., 329
Superstructure, *see* Oberbau
Sutherland, 65, 67; injection complexes, 16–7, 27, 31, 33, 36, 37, 38, 39, 150, 185, 186, 206, 219, 241, 273–4, 293, 306, 347
Sutton, J., 378–80
Suzuki, P., 218, 220, 221
Svecofennids, 384, 387
Swaziland, 357, 358, 361, 364
Synchronous batholiths, 319, 329
Synkinematic granites, 312, 318, 319, 384, 395
Synorogenic sediments, 388, 389
Syntectic rocks, 226–7

Syntectonic imbibition, 232, 320–3
Syntectonic magmatic phase, 311–2, 314–6, 323–4, 326–7, 384, 389
Syntexis, 33, 88

Tache d'huile, 6, 10, 111, 116, 120, 126, 149
Taimyr, Siberia, 284
Tait, D., 49, 52
Tanganyika, 387
Tangential movements, 246
Teall, J. J. H., 17, 21, 58, 280, 308
Tectonic pips, 354, 363
Tectonites, 277–88
Tele-intrusion schists, 9
Temperly, B. H., 378
Termier, P., 5, 10, 21, 33, 39, 111, 112, 161, 162, 193, 200, 211, 251, 326
Termier, H. and G., 390, 392
Thaya, 301–4
Theory of the Earth, 49–52
Thermal gradient, 26
Thermal metamorphism, *see*, contact metamorphism
Thomas, H. H., 36, 137, 153–4
Thuringerwald granite, 101, 151
Thuringia, 311
Thuringia-Saxony block, 311–2
Tilley, C. E., 30, 40, 41, 60, 203, 204, 208, 210, 211, 220, 232, 295
Tillites, granitized, 348
Time dimension in plutonism, 263–8
Tin lodes, 97
Torridonian sandstone, 300
Torridon, Loch, area, 379–80
Tourmaline, 41, 231–2, 356
Traîneau écraseur, *see* creative block
Transformists, 181, 186, 351
Transfusion, 13, 33, 88, 160, 234, 236, 241
Trégastel granite, 137, 153–4
Trends in porphyroblasts, 279–85, 287
Truter, F. C., 349, 353
Tugela River, Natal, 344
Tulle anticline, Massif Central, 290
Tuominen, H. V., 377
Turner, F. J., 37, 42, 205, 212, 220, 221–2, 224, 232, 244, 249, 272–3, 276–7, 281, 288
Twain, Mark, 263
Tweefontein Mine, Namaqualand, 354
Two-magma Theory, 70, 71, 77, 173, 370
Tyrrell, G. W., 60–1

Ultrametamorphism, 10, 32, 89, 118, 119, 120, 123
Ultrametasomatism, 119
Umbgrove, J. H. F., 396, 397
Umgeni valley, Natal, 349, 364
Unconformity, base of geosynclinal sediments, 381–5; significance of, 334
Uniformitarianism, xi, 26, 27, 71, 76, 131, 173, 243, 263, 368
Unst, Shetland, 24, 37, 144–5, 149–50, 154, 186, 188, 218, 219, 224, 272, 274, 297–8, 307, 361
Unterbau, 127, 158, 386, 387

Valla Field, Unst, 297, 307
Van Hise, C. R., 7, 11, 13, 15, 22, 24, 74, 175, 195, 198, 239
Van Rhynsdorp, Cape Province, 353
Variscan orogen, 28
Varves, 24, 31
Veining, 271
Venites, 11, 67, 122, 123, 130, 131, 249
Virlet d'Aoust, 4, 93–4, 95, 158, 162, 325
Vogelgesang, W., 100
Vogt, J. H. L., 137
Vogt, T., 31, 40, 205, 222, 223, 293
Volatiles, 2, 4, 5, 8, 14, 35–6, 61, 108–9, 144, 181, 332
Volcanic Association, 53–4, 97, 105, 165–7, 171, 173, 369, 371
Volcanic Class, xv, 165, 166, 167, 171, 172, 173
Volume Law, 7, 9, 198, 214
Vosges, 98, 102, 260

Wager, L. R., 79–80, 177
Wahl, W., 137, 310, 313, 395
Waldman, L., 272, 276, 303, 304, 308, 321, 323
Waldviertel, 6, 223, 272, 301–5, 308, 321, 323
Walker, F., 188, 349
Ward, Clifton, 16, 56–7, 59, 158, 325, 329

Waters, A. C., 319
Watson, J., 219, 273, 378–80
Weber, M., 4
Wegmann, C. E., 11, 35, 37, 38, 40, 124–30, 131, 144, 149, 151, 156, 157–8, 162, 182–3, 186, 189, 192, 235, 239, 251–2, 254, 256, 288, 310, 313, 328, 330, 332, 377, 386–7, 393, 397
Weinschenk, E., 7–8, 38, 234, 279
Wells, A. K., 46–7, 137, 153
Wenk, E., 243–4
Werner, A. G., xii, 197
'Wets,' xv, xvi, 237, 351
Wiid, D., 354
Willemse, J., 357, 362, 366
Williams, G. H., 58
Williams, G. J., 219
Williamson, W. O., 41
Willis, B., 25
Wilson, Gilbert, xv, 33, 36, 40, 42, 88, 90, 142, 159–60, 192, 393
Wiseman, J. D. H., 203, 288, 294–5
Wissahickon schist, 274–5, 281
Wooldridge, S. W., 137, 153

Xenoliths, 65, 95, 144, 145, 148, 152–4, 156, 233, 236–7, 346

Yugoslavia, 33, 36, 40

Zirkel, F., 58
Zoned enclaves, 233, 235
Zone, of flow, 7; of fracture, 7
Zones, *see* depth zones, ectinites, metamorphic zones, migmatite zones
Zululand, xvii, 344

Works on Geology published by

THOMAS MURBY & CO

PROPRIETORS: GEORGE ALLEN & UNWIN

GEORGE ALLEN & UNWIN LTD

London: 40 Museum Street, W.C.1

Auckland: 24 Wyndham Street

Bombay: 15 Graham Road, Ballard Estate, Bombay 1

Calcutta: 17 Chittaranjan Avenue, Calcutta 13

Cape Town: 109 Long Street

Karachi: 254 Ingle Road

New Delhi: 13-14 Ajmeri Gate Extension, New Delhi 1

São Paulo: Aveuida 9 de Julho 1138-Ap. 51

Sydney, N.S.W.: Bradbury House, 55 York Street

Toronto: 91 Wellington Street West

RUTLEY'S MINERALOGY

H. H. Read

'May be recommended to every student of mineralogy from whatever angle the subject be approached.' *Nature.*
'Still stands alone as the best British work on mineralogy for students of geology.' *Science Progress.*

Cr 8vo 27th Impression 24th Edition 18s. net

IQBAL LIBRARY

METAMORPHIC GEOLOGY

H. H. Read, John Sutton, Janet Watson

This book is intended to meet the need for a text dealing with the complete geological history of the metamorphic and granitic rocks, rather than with their mineralogy, petrography or physical chemistry. Drawing on his lengthy field-experience of the crystalline rocks, and helped by two of his younger colleagues, Professor Read surveys our knowledge of metamorphism as a phase of earth-history. The work throughout treats the metamorphic rocks as components of the earth's crust and as documents of the geological record. This approach involves the consideration of such topics as sedimentation, igneous action and tectonics in so far as they bear on the history of metamorphic processes. It is held that the metamorphic rocks can be best understood by relating them to the environment in which they were produced.

Metamorphic Geology sets out to demonstrate that the study of metamorphism can be made to throw light on the major episodes in earth-history. It will at once be recognized as a work of major importance.

Demy 8vo About 50s. net

OUTLINE OF HISTORICAL GEOLOGY

A. K. Wells

'An excellent general account of British stratigraphy, pleasantly written in the form of a continuous narrative, and very fully illustrated .... It should be appreciated by a wide circle of readers.' *Scottish Educational Journal.*

Demy 8vo 2nd Edition 25s. net

18 30078 7-11-59